

# Contemporary Psychology

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## Call for Dr. Finagle

George A. Miller, Eugene Galanter, and Karl H. Pribram

*Plans and the Structure of Behavior.* New York: Henry Holt, 1960. Pp. xii + 266. \$5.00.

Reviewed by D. O. HEBB

*Everybody knows Dr. Hebb, Professor at McGill University since 1947, chairman of its Department of Psychology for ten years, author of The Organization of Behavior in 1949 (Wiley), trained in neurology by Wilder Penfield and in neuropsychology by Karl Lashley, and right now President of the American Psychological Association.*

THIS is an Essay Concerning Human Understanding, considered as a computer activity. Its purpose is to repair the sins of omission in 'cognitive' psychology by giving more attention to motor output, and in so doing to bring back into psychology things that have been neglected. It is not a systematic explanation of behavior, armored against criticism, nor a scholarly review of the present state of knowledge, though it has valuable elements of both. It is a relatively informal and undocumented intellectual diary, which the authors think will irritate as well as enlighten. They are right on both counts, but, if the book is seen for what it is, the enlightenment will come out well ahead of the irritation.

What may irritate the reader is that the authors, preoccupied with some very good ideas, rather misrepresent what

has been done by others in the same field. Besides, they have left some loose threads of their own which, when pulled by the reviewer, seemed to endanger the whole fabric: in several respects the book shows signs of haste. But it still has much to offer. The writing is excellent; something genuinely new is presented here concerning man's mind; and new light is cast on old issues. This is the best computerman's study of behavior that has appeared, one of the rare ones that tell us something worth knowing about human beings.

The book is the product of a year's discussion at the Center for Advanced Study in the Behavioral Sciences. To write a systematic treatise covering the same ground might well need five years, and this had to be completed in one: one can understand that the synthesis of such divergent points of view was hardly to be worked out by mail after the authors had returned to their separate academic posts. All three of them are psychologists, but they arrived at that estate by different routes and retain rather different interests: Miller, the student of language and information theory, marked with the positivism that Harvard's Department of Psychology practices if it does not always preach;

Galanter, the learning theorist, an S-R man, with a preference for keeping the black box (the skull) closed; and Pribram, the neurophysiologist and neurosurgeon, whose whole career has been spent prying it open. These three have reached agreement—by no pacific path, they suggest, but still agreement—on a cognitive psychology. It is cognitive with a difference, but the S-R formula is renounced and the new scheme puts images and volitions, instincts and plans, squarely in the center of the stage.

Two key conceptions are the *Image* (this one credited to the economist, K. E. Boulding) and the *Plan*. The Image with capital I is not just an image, but all the ideas and knowledge that S has of himself and his world. The Plan varies from time to time; it consists of the processes that control sequential behavior, and it is important to recognize that it has a hierarchical structure, with the details subordinated to larger objectives and brought in only at the proper time.

These are very molar conceptions indeed; in other words, they are vague as to detail, and they are not worked out consistently to the point where one could consider this to be a theory of thinking. An atmosphere of hard-boiled fact-mindedness is created by the parallel between Image and the stored information of a computer, and between Plan and the program that controls the computer's operations. Like any other analogy, this has both value and limitations.

In their enthusiasm for the new ideas, the authors fail to do justice to some old

ones. They throw out the S-R paradigm in a few sentences at the beginning of the book, for example, as if it had no further relevance or were just an unfortunate historical mistake; whereas what they are really doing is to improve the paradigm by recasting it in cybernetic terms, and extending this 'mechanistic' conception of behavior by showing how to incorporate the representative and purposive function in the same single scheme. They reprove the cognitivists for failing to deal with response: a criticism which applies to some of them, but not to Lashley, who was a cognivist, and not even to Tolman, who is criticized specifically. The authors should look again at *The Determiners of Behavior at a Choice Point* (E. C. Tolman, *Psychol. Rev.*, 1938). The *Cognitive Map* (*ibid.*, 1948) was explicitly defined as a determiner of response. Where is it defective? In not specifying how it integrates incoming stimuli into the controlling central process, nor how the control of effectors is achieved. But the present writers do no better. If Tolman's map sometimes suggests a ghostly map-reader, the Plan—as we shall see—seems at times to imply a maker or utilizer of plans; we are never shown the origin of the Plan, nor its mechanism of motor control. Both conceptions are and remain at the level of analogy.

Again, psychology is condemned for losing sight of the importance of *intention*, a synonym for purpose, which most definitely has not been lost sight of. The treatment of the comparative problem is misleading; it is not possible today to regard instinct as uninfluenced by learning (F. A. Beach, *Psychol. Rev.*, 1955), and ethologists do not so regard it. The authors seem unaware of what has been done on emergent phenomena and the development of language; for example, see J. A. Bierens de Haan (*Biol. Rev.*, 1930), T. C. Schneirla (*J. abnorm. soc. Psychol.*, 1946), or D. O. Hebb and W. R. Thompson (G. Lindzey's *Hdbk. soc. Psychol.*, 1954). The gap between man and ape is great, but not as great as between man and three-spined stickleback, the only two levels that seem taken into account.

Now the main proposals. What values are there in drawing the parallel between the brain and the man-made computer? First, as important as any, one may wish to dispel mysticism; in this case, to show that seeking or purpose can be a property of a mechanical, deterministic device. Secondly, one might try to improve computers, by finding



Three authors in search of a plan: from left to right, KARL H. PRIBRAM, EUGENE GALANTER, and GEORGE A. MILLER.

out where and how the brain does it better; or, thirdly, the same comparison may achieve a better understanding of brain mechanisms themselves. Ostensibly, it is the last that is in question here. When one looks for details, however, one does not find them. There is an effective use of the similarities between mental process and computer action, but not that search for differences, or definition of the limits of similarity, which would make this into more than analogy; and so it seems that what the authors do is to present us with certain valuable conceptions—I emphasize this—but use the comparison mainly to justify the conceptions, not to develop them further: it is all right to talk about plans and intentions, it is not animistic, because machines can have plans too.

I may not be doing justice at this point. The authors' discussion of language, for example, is probably clearer and more cogent than it would be without the computer comparison. But the reader will not find the larger question explored: how do computers, as we know them now, differ from the thinker? If they do not differ in principle, the problem of thought is solved; if they do, and we can say where, we have made another step toward defining the central problem in the understanding of man.

The most question-begging aspect of the book has to do with volition and the initiation of the Plan. At times one cannot avoid the suspicion that the authors' thinking still has touches of animism, and of the ghostly switchboard operator. S "has access to . . . Plans that he might execute if he chose" (p. 62); "as soon as the order to execute

a Plan has been given" (*ibid.*); "when the decision is made to execute" a Plan (p. 65): these phrases are at the best careless. Who or what chooses, decides, gives orders? The authors when challenged, I think, would ascribe this function to a Metaplan, which I also think would be legitimate; but such slips vitiates the argument considerably. On page 112, the hypnotized S is "forced to relinquish the planning function to another person": how can one say this of an essential part of the machinery, the program tape? Is the (human) computer now functioning without its tape, its activity in detail controlled by the tape that rests in another computer? Or is it instead that the tape of a Metaplan is changed?

Closely related is the question of volition, on which the authors have not made up their minds: an effort of will is emphatic *inner speech* (p. 71), volition is the *capacity to execute* Plans (p. 111), an act of will is *creating* a plan (p. 110).

A significant omission concerns the nature of the information that is fed into our computer: in other words, the question of perception, equivalence, abstraction is pertinent. There is, however, nothing in this book even to suggest that the problem exists. This question is known to be the stumbling-block for neural theory, and it may be the reason why the authors have stayed as far from the real nervous system as they have. The final chapter on the nervous system is appended almost as a separate essay. It is an interesting and useful discussion, but it says little about mechanisms. The CNS which it describes is the anatomist's, not the neurophysiologist's. Where a "working memory" may exist is discussed, but not how. So we have a kind of black-box CNS, with the outer layer of scalp and skull peeled off to reveal gross structure, but with no further prying into the operation of its finer parts.

FINALLY, there is the topic of creativity in problem-solving. Here the computer comparison does indeed help to define the essential issue, but by exclusion. The authors lead right up to it and then shy away. They explore problem-solving in terms of the search para-



digm: it consists of trying out a very large number of answers internally, as a computer does, until one is found that fits. They quote Poincaré in support: "Discovery is discernment, selection" (p. 167). They conclude that the explanation is not sufficient because the number of items to be searched is too large; but they do not observe that Poincaré was talking about *creating*, and that his selectivity was "in not constructing useless combinations" of ideas. This is something quite outside the scope of computers, as known at present.

It is said that a Dr. Finagle really defined the difference between brain and computer by defining a constant which, when added to, subtracted from, multiplied by, or divided into a wrong answer, will turn it into a right answer. Finagle proposed that the only known source of this constant is the human brain. In other words: give a computer wrong or incomplete data and it returns a wrong answer or none, whereas the human brain characteristically operates with such data and surprisingly often, by a creative leap, comes to the right answer in defiance of logic. This is the problem of creative thought *par excellence*, the one on which we really need light. The only contribution of the computer here is in showing what thinking is not.

It is not just, however, to criticize the authors for not solving this problem. They may leave the impression that brain and computer work on the same principles, but they do point out (p. 188) that the brain is still enormously better than the most complex of existing computers. Their admirably clear discussion will help the reader to define his dissents, if he does dissent (after all, they set out to provoke argument). And readers of very diverse interests will find this a stimulating as well as a clarifying book, one to be read and digested.

Though the authors abjure S-R theory, the man whose interests lie here will find a valuable analytical tool, not too foreign to his thinking, in the TOTE conception (a cybernetic unit of analysis to replace or to amplify the reflexive response). It provides a much more adequate account of 'the' response, as

a temporally organized sequence. The whole discussion of skills in Chapter 6, or of memorization in Chapter 10, shows how useful the Plan conception is. There are significant contributions to the current literature in bringing Lewin back to 'experimental' psychology (in the narrower sense), in relating hypnosis to the same body of thought, and in the excellent analysis of the control of speech production and the "grammar of behavior." One danger of such works, too often realized in the past, is that a good explanatory

conception may narrow the universe of discourse and blind the thinker to the wealth and complexity of behavioral phenomena. The Plan and the Image have not so blinded Miller, Galanter and Pribram; these ideas instead have broadened their views (as of record) and ought to do the same for others.

This is a valuable, stimulating and enlightening Essay. We may hope that the authors will go on with their joint enterprise. It should be a beginning, not an ending.

## English Psychiatry's Revolution

Vera Norris

*Mental Illness in London.* (Maudsley Monographs, No. 6.) Pp. 317. 35s.

Michael Shepherd

*A Study of the Major Psychoses in an English County.* (Maudsley Monographs, No. 3.) London: Chapman & Hall, for the Institute of Psychiatry, 1957. Pp. 159. 25s.

Reviewed by JOSEPH ZUBIN

Dr. Zubin is Chief of Psychiatric Research in Biometrics of the New York State Department of Mental Hygiene and also Professor of Psychology at Columbia University. He is the author of many articles and several books: with P. H. Hoch, *Psychopathology of Childhood* (Grune & Stratton, 1953; CP, Mar. 1957, 2, 63f.), *Experimental Psychopathology* (idem, 1957; CP, Apr. 1958, 3, 106f.), *Psychopathology of Communication* (idem, 1958; CP, Oct. 1959, 4, 334f.), and, with 8 others, *Current Trends in Description and Analysis of Behavior* (Univ. Pittsburgh Press, 1958; CP, July 1959, 4, 214f.). He has reviewed *Washington University's symposium on Theory and Treatment of the Psychoses* (Washington Univ., 1956; CP, July 1957, 2, 185f.).

MODERN hospital psychiatry may be said to have begun in the nineteenth century with the ushering in of 'moral therapy' under the leadership of such men as Pinel in France, Rush in

the U. S., and William Tuke in England. Just why moral therapy declined is difficult to fathom unless one looks to such obvious factors as the industrial revolution, urbanization, and, in the U. S., immigration which overwhelmed the economy as well as the hospitals during the latter half of the nineteenth century. At the beginning of the third decade of the twentieth century, the tide turned again. The rise of the somatic therapies was heralded by Wagner-Jauregg's malaria treatment for general paresis in 1917. The shock therapies followed in the '30s and '40s, but the introduction of these therapies in England was gradual. Not until the end of the Second World War did the new trend become apparent. The two books now under discussion ring out the old era of static psychiatry and ring in the new postwar period. The changes are documented with statistical data gathered in mental hospitals in England since the early '30s.

Shepherd, in a tersely written but

meaty book, examines hospitalized cases of mental illness in a small, prosperous, and stable county with a population of 400,000, served by only one hospital and evenly balanced between urban and rural residents. He contrasts the admissions from 1931-33 with those from 1945-47, following both groups for 5 years. The periods were selected so as to compare the 5 years preceding World War II with the five years following World War II, with the war period itself eliminated.

Norris, in her posthumous book, describes the story of mental disorder from the period 1947-1949 up to 1951, permitting an overview of the initial impact of the National Health Act which began in July, 1948. She studied admissions to two observation (or receiving) units and three mental hospitals in three areas of London. The patients were drawn from a population of 450,000 to 600,000. Both Shepherd and Norris suggest that their samples were representative of the national picture in hospitalized mental illness.

It is fortunate that these studies focus on the above periods since they mark the end of an area in the statistics of mental disease. These studies have special significance for the United States, for England appears to be about ten years ahead of America, not only in experience with the open hospital policy, but also in the application of new policies of admission and release. Thus in England the trend of increasing admission rates and declining resident rates, which we in the States are just beginning to experience, began about a decade ago. Hence the established British pattern might very well represent what we can expect in care, cost, and distribution of mental illness in this country in the next decade.

Before making such comparisons we had, however, better realize that comparative psychopathology, like comparative anatomy, can not be dealt with in an offhand fashion. It was not until anatomists began to compare organs with reference to their functions rather than their forms that comparative anatomy became meaningful. Although the amount of basic psychopathology may be biologically invariant from culture to culture, the characteristic behavior

may be more fully elicited in some cultures, occluded to some degree in others, while in still others it may even be exploited to the benefit or detriment of the individual or the group. For this reason the drawing of comparisons between incidence figures in different cultures, even though the cultures are as closely alike as those of Great Britain and the United States, is hazardous.

**W**HAT are some of the conclusions emerging from these two studies? They are of two types: (1) those dealing with such specific aspects as admission policies and management, which are largely dependent upon changes in law and in available facilities, and (2) more general conclusions about intrinsic relationships among sociocultural factors, diagnoses, outcome, etc.

Shepherd found, for the period prior to the introduction of the National Health Act, a marked increase in admissions and in releases for all conditions but schizophrenia, but no increase in hospital beds. This could only have happened as a result of shortening the stay of the average patient. Whether the increase in release rates was occasioned by the new therapies, by changes in community attitudes, or by change in type of patients admitted is difficult to determine. Probably each of these factors contributed its share. The new therapies alone could not explain the change because the most frequently used new therapy in Shepherd's hospital was continued narcosis, a technique which is now known not to be highly efficacious. There is some evidence of a change in the character of the inmates. During the second period the population was older and included higher proportions of females, of married persons, of voluntary patients, and of readmissions. There was an increase in the number of diagnoses of functional psychosis, in particular of affective disorders. All of these changes in population bring in patients with somewhat better prognoses. It is likely, therefore, that the changes in release rates are due, at least in part, to the better prognosis of the newly admitted patients. But why did this new influx of patients occur? Perhaps there was a

change in attitude towards hospitalization for mental illness even before the passage of the National Health Act.

One of the most surprising findings in Shepherd's study is the reduction in absolute numbers of first admissions for schizophrenia, as well as in rate, in the second period. Whether this difference represents a change in diagnosis, a change in the type of facility to which schizophrenics first came, or some statistical artifact—such as variability due to small numbers—is difficult to determine. The number of readmissions per schizophrenic patient, however, rose, while the total duration of residence in the hospital during the follow-up period declined. It seems that a change in policy, at least, with regard to schizophrenia had occurred. Instead of resembling a prison, as it did earlier, the hospital during the second period became more like a subway train—always full but never with the same people, although the same passengers get in and out at various time periods. Apparently the English hospitals became not so much open-door as revolving-door hospitals, recycling their populations in and out.

A good way to study such a revolving population is by applying an index of retention in which the number of days of actual hospital residence is related to the total number of possible days of residence during the period beginning with the admission of the patient and ending with the conclusion of the follow-up period—with some weight also given to the number of readmissions for each patient. Examples of such indices are the Immobility Index (Crandell et al., 1956) and the Outcome Index recently developed by Burdock (Zubin, 1960a).

**N**ORRIS' is one of the best studies of the treatment of mental disease because it starts off with specific definitions of terms and uses standard actuarial and statistical methods. She developed several new indices, including a "follow-up success rate" for the proportion of time following admission to the hospital during which the patient was followed. Patients who were followed continuously up to end of the survey were rated at 100% success in follow-up, while those

who were lost to observation either because of death or release before the end of the follow-up period were rated proportionately less successful in follow-up. About half of the patients remained in the hospital throughout the period of follow-up, thereby attaining a score of 100% follow-up success. The follow-up success rates of the rest varied; seventy percent was arbitrarily selected as a satisfactory rate. It should be noted that much more reliable information was available for the patients who failed to leave the hospital than for those who were released.

As data for the period previous to July 5, 1948, are unavailable, it is difficult to measure the impact of the National Health Service on the utilization of admission wards and hospital beds. However, it is interesting to note that the number of individuals referred to the authorized officer at County Hall for mental conditions rose from 4,500 in 1946, to 7,800 in 1952 while the proportion of such persons against whom no action was taken under the Lunacy Act rose from approximately zero before the introduction of the National Health Act, to 22% in 1952. Apparently more individuals were taking advantage of the availability of mental-health facilities, with larger proportions of those wishing admission referred to places other than a mental hospital or being found not in need of treatment.

A rather striking contrast between England and the U. S. A. is found in the frequency of manic-depressive psychosis. Far from being a vanishing condition, as is the case apparently in many parts of the United States, it is quite common in England. Although the prognosis in manic-depressive psychosis seems to be more hopeful than in schizophrenia, it is, nevertheless, a severe condition. About half of those admitted with this diagnosis spend less than 16 weeks continuously in the hospital, while 9% spend at least 4½ years continuously in the hospital. Whether these last would continue to be labeled manic-depressives in the U. S. is doubtful. The others have one or more recurrences of the illness in the course of their lives, so that on any one day in England, one in every 1,000 persons aged 16 or over is resident in a mental hospital on

account of manic-depressive psychosis. The ratio of number of cases in the hospital on a given day to the total number of patients in the country is unknown, but according to Scandinavian estimates hospitalized manic-depressives are only 14% of the total number.

As for the mental disorders of old age, the expectation at birth of being admitted to a mental hospital at least once after the age of sixty is 2% for men, 3% for women. For those admitted the outlook is gloomy, early death being the most usual outcome. The presence of affective symptoms is correlated with an increased chance of being discharged to the home. The mortality rates are higher for men than for women, the greater proportion of women remaining in the hospital as long-term patients.

The mortality rates for the diagnostic groups indicate that schizophrenics, who are usually regarded as psychologically immature, age much faster than the general population. The average age of the schizophrenics was 33 for males and 38 for females, but their mortality rates corresponded to those of ages 54 and 65, respectively. That is a disadvantage of from 21 to 27 years vis-à-vis their age-peers in the general population. Manic-depressives similarly show a 20-year handicap in mortality rates, while those suffering from old-age psychoses have a 15-year disadvantage. Perhaps all three of these disorders have a common organic factor. In this connection, it is interesting to note that interference with oxygen metabolism has been implicated in the aging process, while it has also been repeatedly alleged that the mentally ill have some metabolic deficit.

**W**HAT are some of the more general results that have emerged? First of all, the incidence of mental illness is found not to vary much among the United States, Great Britain, and various other western countries. Secondly, the incidence rates for specific illnesses, such as schizophrenia and manic-depressive psychosis, differ even less than the over-all rates for all mental disorders. Thirdly, the persistence with which the diagnostic label clings to a patient differs somewhat from country to country.

In England and in the European countries generally the diagnoses of manic-depressive psychosis and schizophrenia have a high degree of adherence, whereas old-age psychoses seem to have less adherence. The organic psychoses have the highest degree of adherence, while the neuroses have a rather low degree. In general, diagnostic labels in the United States are not so firmly adhesive as elsewhere. This, of course, raises an issue which ought to be looked into more carefully. In a recent Work Conference on field studies in mental disorders (Zubin, 1960b) steps were proposed for increasing the consensus on diagnosis. It was concluded that further conferences about differences in diagnosis would not lead far, but that an actual field investigation in which psychiatrists, psychologists, social workers, anthropologists, and biometricians from the various cultures would participate, might be useful. One suggestion was for surveys of the registrants of military service in the various countries.

The comparison of incidence figures is impeded by another difficulty. In many instances the figures are for first admissions to mental hospitals, not for true incidence in the population. It is even difficult, however, to compare first admissions because they are defined differently in various places. For example, in the studies being reviewed, the patients who formerly had treatment for mental illness in places not recognized as mental hospitals were not counted as readmissions but as first admissions, even though in other countries they would have constituted readmissions.

One of the most striking features of Norris' book is its rather pessimistic note about the outlook for mental patients once they enter mental hospitals. In the major categories, only one-fifth or one-fourth of the patients were released permanently, whereas the majority had at least one readmission during the four years following first admission. It is important to delve further into this problem by providing better base lines from which to make prognoses for the variety of patients entering our hospitals. It is possible that if we were to make a biometric assay of each patient as he came in and to follow his career through treatment,

we might discover that certain types of patients are more suited for certain types of treatment than others. Such a refinement in treatment procedure might lead to greatly improved outcomes. The battle of prognosis would, moreover, be greatly advanced, were it attacked simultaneously on three levels: pre-morbid, morbid, and course of illness. These three types of information in combination might lead to a more accurate prediction of outcome. Moreover, outcome on follow-up cannot be based merely on in-or-out-of-hospital status. There must be definite attempts at finding the patient in the community and seeing how well he has adjusted. With the marked increase in release rates, the number of former patients adjusting or not can not be determined by the fact that they are out of the hospital. Only follow-up observations with techniques and tools specially developed for evaluation of degree of adjustment can answer this question. This is the greatest need of the moment and efforts to meet this need are almost certain to be rewarding.

THE two studies end in 1952, just prior to the discovery of chlorpromazine and the introduction of drug therapy on a large scale. Here again, it is noteworthy that England has not used the drug therapies as extensively as the U. S. Perhaps the open-door policy and the smaller hospitals made the universal use of drugs unnecessary. Whether the American experience with drugs will nullify the poor prognosis described by Norris remains to be seen.

Another current problem raised in these volumes is that of providing more facilities for the care of mental patients. On this point Norris has the following to say:

In the Annual Reports of the Board of Control the gravity of the situation is stressed yearly, as in the Report for 1953 in which is stated:

"The hospitals were overcrowded to the extent of 18,923 patients. References have been made in previous Reports to the expedients adopted to cope with this overcrowding and with the deteriorated conditions which have resulted."

Such a situation obviously calls for more facilities and more efficacious methods of treatment and prevention. The former lies

in the hands of the Ministry of Health, responsible for hospitals and specialist services; the latter is the function of research workers who need the support of relevant organisations but who ultimately succeed or fail, contribute much or little, according to their gifts and experience in this complex field of inquiry. It is not for the medical statistician to draw up a programme of psychiatric research or to tell Governments how to find the money and materials for building new mental hospitals and how to recruit sufficient nurses and other staff. He may, however, properly draw attention to the urgent and in some ways alarming picture which his investigations uncover—a picture which many psychiatrists would suppose overdrawn, because of their clinical impressions regarding the effects of modern treatment, and which others would prefer to minimize for fear of undermining the confidence that the public has been developing during the last decade or two in the benefits of mental hospital care. Public confidence in mental hospitals is valuable, but it is more secure and likely to be retained if with the proof of the many demonstrable benefits of treatment, serious evidence of our general deficiencies in preventive and therapeutic knowledge is honestly stated and frankly discussed. The findings of this survey are intended to contribute to the objective appraisal of the present position, broadly in respect of the course and outcome of illness requiring mental hospital care, and the strength of some factors which evidently influence it.

If the English trends are truly predictive of what will happen in this country, the proposed contraction or expansion of mental-health facilities—in mental hospitals, day hospitals, clinics, general hospitals, and community agencies—ought to be reviewed cautiously.

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## Feeble Science for Sale

Herbert I. Abelson

*Persuasion: How Opinions and Attitudes are Changed*. New York: Springer Publishing Company, 1959. Pp. x + 118. \$3.75.

Reviewed by WALTER WEISS

who is Associate Professor of Psychology in the Graduate School of Boston University. He was one of Carl Hovland's PhDs at Yale in 1952 and his interest has remained centered on research on opinions and attitudes ever since.

IN 1956 the Opinion Research Corporation distributed copies of a monograph, entitled *Some Principles of Persuasion*, that had been prepared by their Chief Psychologist, Herbert I. Abelson. This survey of the available literature on attitude change was one of a series in "practical applications of social science to business problems." It was written in a nontechnical, unjargonized style designed to carry to public-relations people the 'wheat' of the research relevant to their task of persuading publics. The present volume is an updating of the previous paper-covered one, with the same format and focus on the practical needs of professional communicators. The emphasis throughout is on how to change opinions and not on an understanding of the processes relating to the formation, maintenance, and change of opinions (or attitudes) or of their personal and social relevance. No integrating conception is applied to the surveyed literature. Practical utility is the guide for the selection of reported research and the extraction of persuasive principles. What cannot fit the judged needs of communicators is omitted.

The preface makes clear that the volume is intended for practical people who, while acknowledging that research may provide useful information, still lean on limited personal experience and intuitive rules of thumb for their guid-



ing principles. For after noting some of the advantages of scientific methodology, the author softens the impact and implications of his remarks by stating: "Under slightly different conditions, an experiment might have turned out differently. Therefore, the material that follows should be regarded by the reader as something to compare with his own experience, rather than as a substitute for experience."

This text is made up of a succession of units of the following kind: a simply worded persuasive principle is presented; it is followed by summaries of "specimen studies" from which the principle was extracted; then a brief discussion amplifies, elaborates, or qualifies the implications of the reported research. Sometimes when research is inconclusive or inconsistent, the author will offer his own suggestions on how the variables could be utilized in persuasion. This 'going beyond the evidence' to make even equivocal findings useful is consistent with the narrow practical focus of the volume.

Many of the stated principles of persuasion are well written and relate in simple language what a set of studies is about. For example, "when opposite views are presented one after another, the one presented last will probably be more effective" (primacy vs. recency); "when the audience starts out disagreeing with you, or when it is probable that the audience will hear the other side from someone else, present both sides of the argument" (one side vs. two sides). As might be expected, considering the purpose of the volume, the summary principles and the reports of specimen studies often round off the complexities and qualifications contained in the original literature.

Throughout the reported studies are used uncritically. Nor does the varying quality of research seem to be reflected in the author's remarks concerning the research. The public to whom this volume is addressed will not care, yet why does the author use secondary sources as references instead of the accessible primary ones?

## Measurement; the 1956 Models

C. West Churchman and Philburn Ratoosh (Eds.)

*Measurement: Definitions and Theories.* New York: John Wiley & Sons, 1959. Pp. viii + 274. \$7.95.

Reviewed by VIRGINIA L. SENDERS

*who is Lecturer in the Department of Psychology at the University of Minnesota and author of Measurement and Statistics (Oxford Press, 1958; CP, Aug. 1959, 4, 231f.). In her past, if you go back far enough, lie Mt. Holyoke College, Harvard University, Wellesley College, Wright Field, and Antioch College, and the thread of interest in measurement runs all along.*

At the meetings of the American Association for the Advancement of Science in December, 1956, specialists from the disciplines of economics, accounting, physics, philosophy, and psychology participated in a five-part symposium on fundamental problems of measurement. Their thirteen papers, regrouped by the editors into four sections on the meaning and the theory of measurement, and problems in the physical and the social sciences, are published in this book. "The result," say the editors, "is a book of contrasts. This volume does not constitute a series of connected discourses on measurement. It is not a textbook on the subject of measurement. But it may present as good a picture as possible of what the workers in the foundations of measurement are concerned about in the middle of the twentieth century."

The four papers that will be of most interest to psychologists all deal, wholly or in part, with the relationship of psychophysical scaling to the measurement of utility. Such unanimity of interest undoubtedly made for an integrated symposium, but it can hardly be considered representative of psychologists' concerns about problems of measurement. In this respect the books fails to fulfill one of the editors' expressed

hopes for it. But the contrasts are surely there. Taken together (and it is the reader who must bring them together, for they are in three separate sections of the book) these four papers provide a provocative diversity of approaches to a single topic.

For S. S. Stevens, utility measurement is the last topic in an article which also includes a historical summary of modern controversies about measurement in physics and psychology, reviews the scales, introduces the newest one, the logarithmic interval scale, and explicates the nature of "prothetic" and "metathetic" continua. Utility, Stevens speculates, may well be a prothetic continuum, upon which just noticeable differences are not subjectively equal. If that be so, then scales derived in the Fechnerian tradition of inferring psychological distance from inconsistency of choice will not accord with those obtained by methods of direct appraisal. Is direct appraisal of utility possible? Stevens thinks so and suggests that those who would derive scales of utility might aim directly for ratio scales by using such procedures as asking subjects how much money it would take to make them twice as happy as ten dollars would make them.

Coombs makes quite a different kind of attack upon the applicability of the law of comparative judgment. His paper is the report of an experiment, complete with photographs of the apparatus, tables of data, and tests of significance. His results lead to the conclusion that the relation of inconsistency of preferences to psychological distance is dependent upon the variance of the subject's distribution of ideals.

ALTHOUGH the law of comparative judgment is thus under fire from two different directions, it is nevertheless of crucial importance to the formal models for utility measurement presented by Luce and by Davidson and Marschak. Both of these papers, written in the formal language of definitions, axioms or primitives, theorems and proofs, provide impressive theoretical structures. The latter essay also cites the results of a betting experiment with seventeen subjects as evidence that the model is at least superior to one based upon

actuarial values of wagers or upon the degree of risk involved in a choice.

Unfortunately these four papers will probably not appeal to a single audience. Stevens' paper should make an interesting, though unconventional, introduction to utility theory; most of the other topics in it have been covered by Stevens in the periodical literature since the symposium. The papers by Luce and by Davidson and Marschak, because of their rather formidable formal language, and because of the relation of the latter to an ongoing research program, appear to be directed toward the expert; yet for the expert Luce's model will not be new, since it has already appeared both in the periodical literature and as part of a book. Coombs' article stands independently as a straight experimental report, but its importance is best appreciated when it is seen in the context of utility measurement and decision theory.

As for the other nine articles, the subject matter of many will interest those psychologists who like to grow in their understanding of the theory and philosophy of science. The table of contents whets the intellectual appetite, but, like a man presented with a boiled lobster and no fork, many a reader will find himself unable to derive much nourishment from the rich fare presented. A few of the contributors have written for all thoughtful scientists: Suppes, for example, has made his English text so clear that his precise formal language will hardly diminish the wide appeal of his discussion. Most of the articles, however, are prepared by specialists for other specialists in the same fields. Physicists write to physicists and mathematicians to mathematicians. In his own paper, Churchman says: "The more precise a language is, the less broadly it is understood. . . . Precise languages narrow the class of users but increase the degree of refinement that any user can attain. The proper balance between depth and breadth is the linguistic decision problem of measurement." It is also the problem of *Measurement*, the volume under review.



## The Couch's Jehovah

Leon J. Saul

*Technic and Practice of Psychoanalysis.* Philadelphia: J. B. Lippincott, 1958. Pp. xi + 244. \$8.00.

Reviewed by EMANUEL K. SCHWARTZ

*Dr. Schwartz is Assistant Dean of the Postgraduate Center for Psychotherapy in New York City, an institution established to meet the needs of patients who cannot afford to pay private fees, the largest outpatient center of continuous treatment and training in the world. He is also in private practice both in New York and in Mexico City, and he writes many articles about the training of psychoanalysts and the practice of psychoanalysis.*

PSYCHOANALYSIS cannot be discussed without mentioning Freud who has, of late, acquired a new surname. Partisans for and against continue to refer to "the master" as *Freud himself*. It is unlikely that you will hear *Shakespeare himself*, *Einstein himself*, or *James himself*, but in psychoanalytic circles *Freud himself* is a commonplace. The primary purpose of psychoanalysis is to understand, yet so little effort is made really to understand Freud and Freudian psychology, and so much is devoted to attacking or defending them.

Most neo-Freudians and non-Freudians seem less interested in understanding Freud, patients, themselves, or human behavior than in destroying Freud. This book will therefore be rejected by most of them. Saul, eminent psychiatrist and training analyst of the Philadelphia Psychoanalytic Institute, tries not to distort or demolish Freud. More significantly, he emphasizes the importance of organized thinking, rationality, and science. He iterates with Freud, "Where the id is, let the ego be," where chaos, irrationality, illusion, impulsivity, and compulsivity are, there let reason, reality, discipline, and freedom be.

Recognizing that theory and practice are related, a brief discussion of psy-

choanalytic theory is presented. No attempt is made to explore all of its facets, and this is good. Moreover, it has been more adequately described elsewhere (for example, by Ruth L. Munroe, *Schools of Psychoanalytic Thought*, Dryden Press, 1955). Saul's focus is practice, how to make sense out of an extremely complicated set of processes, the techniques and mechanics of doing psychoanalytic work with neurotic patients. Somewhat overcautiously he warns that his approach has nothing to do with the treatment of psychotic patients or analytic candidates, trainees.

It is a little too late in the development of psychoanalysis rigidly to propose so puristic (though systematic) a point of view. Diagnostic variations tend to fuse, and accumulated experience in the application of psychoanalytic principles to the treatment of borderline and schizophrenic patients cannot be disregarded. A more tenable position might be the classification of patients in terms of availability of ego resources, ego integration, positive or healthy potentials. Saul is aware that ego strength is largely determinative in successful outcomes and that the ego is the keystone of psychoanalytic practice. Moreover, by this self-imposed limitation Saul sells himself short in underestimating the value of his contribution.

MANY psychotherapists, psychoanalytically oriented and otherwise, are often quite as chaotic in their work as their patients. Adherence to a school or point of view is often blind assertion with little concrete systematic thought back of it. Saul presents models for organizing the wealth of human material which confronts the clinician who seeks to help other human beings with their

living. The young (and even the experienced) analyst needs to learn, among other things, how to understand covert or latent communication, including dreams; how to work out the central psychodynamics of each person; how to perceive and interpret repetitive patterns of behavior, including transference reactions and demands, as they manifest themselves genetically, currently, and in the relationship with the therapist; how to assay the positive and negative potentials; how to recognize difficulties provoked by the analyst; and how to aid the patient to work through, in his thinking, feeling and doing, to a more realistic adaptational mode of living.

Many teachers of how-to-do are vague and stress rather what-not-to-do. Saul offers positive precepts and clinical illustrations. He suggests organized approaches to many situations in which arises that most frequent question of the clinician working with another person or a group of persons over an extended period of time: "And now what do I do?"

This is obviously no plea for a do-it-yourself training as a psychoanalyst. The education of the analyst goes beyond learning sets of rules. A personal analysis—that is to say, experience as a patient—and controls—and that is to say, supervised experience as an analyst—are requisite. Most training programs include also lecture courses, continuous case seminars and clinical conferences. Rules of thumb, highly conceptualized approaches, can, however, be too rigidly applied. It has been said that the occupational hazard of the psychoanalyst is the God-complex, the feeling that one is omnipotent. This can be encouraged by a misuse of neatly packaged, specific directives. Saul suggests that the more you do it (psychoanalytic therapy) the more confident you become. But it is also true that the more confident you become, the less uncertain you are; and the less uncertain, the less questioning; and the less questioning, the more authoritarian. The principles, *Cleave to reality! and Keep an open mind!* help, but too often you cannot make these principles stick, or at least you cannot get clinicians to stick to these principles.

A psychoanalyst is not a nonpartici-

pant observer; he participates. The patient and the analyst have separate histories, but the analysis, too, has a history created by the interaction between analyst and analysand, part of their individual histories, and a factor in the continued interaction. Psychotherapy is a relationship between two persons in which one of them understands, is conscious of what is going on, hopefully the analyst. It may be that we shall need a superanalyst to understand really what is going on and what has gone on in the therapeutic interaction. Research



LEON J. SAUL

designs will be more promising only as the clinician makes precise formulations about what he is doing and why he is doing it in each situation. Saul's conceptualizations of practice represent a significant step in that direction.

For example, an important technical principle in understanding patients is to avoid becoming fascinated by peripheral detail and to focus on main issues. For Saul there are only ten such *Leitmotive* in normal and abnormal development, and the essential dynamics of the total personality must be understood in a single interview. Saul, who has a flair for pithy clinical pictures, claims, "The nuclear dynamics can be discerned in the first interview in, at a rough guess, 85 per cent of the patients seen by the author." What a courageous and challenging statement—calling for objective verification! Of course, "speed in dy-

namic diagnosis and caution in analytic involvement complement each other."

**T**HERE are other ideas that stimulate exploration and investigation, such as evaluating the intensity of treatment as reflected in frequency of sessions per week (the current range is from one to six sessions) and in duration (from one to thirteen years); measuring the outcomes of treatment; accounting for failures; and exposing the nature of addiction to psychoanalysis.

Psychoanalysts tend to be imprecise and use vague descriptive words as if they were measurements; for example, "optimal intensity" of the relationship is based upon maintaining "the optimal transference," which results from the "optimal frequency" of visits; or the infantile id, ego and superego must be replaced by "the mature id" (a contradiction in terms), "the mature ego," and "the mature superego." Perhaps in each instance objective criteria for *optimal* or *mature* do exist, but how is the student to whom his lectures are addressed to know them if Saul does not make them explicit?

The nature-nurture conflict is still a central issue in psychoanalysis. Does the analyst deal with intrapsychic or interpersonal processes, and how are they related? Are the experiences from conception to birth, from one to seven, and from seven to seventy of equal significance? Cultural, racial, social, and economic factors "must be neither neglected nor overvalued, but given their proper weight." How much is proper? This book deals almost exclusively with intrapsychic dynamics as if the external world did not exist. The index has twenty entries with regard to dreams and only one concerning environmental factors.

We can all agree that prevention is a core problem of those who work toward alleviating human suffering. We need not agree, however, that for the emotional "well-being and now even the existence of humanity, nothing is so vital in the long run" as the preventive applications of psychoanalysis. Is not this view too sectarian? Saul states, "no inner peace, no social peace." But what about the reverse? Is mental health possible in a warring world? Where there is social

conflict, there can be no individual security. Perhaps, "no social peace, no inner peace," is as one-sided but just as often true.

Of necessity, no single volume on the techniques of therapy can offer more than help in organizing the experience with patients, each one of whom is

unique in his necessities. The generalizations provided here are extremely useful to the clinician learning to do analytic work, to the teacher of these skills, and to the research worker interested in investigating the meaning and usefulness of psychoanalytic practices. The art of becoming and being is long, and a lifetime is all too short.

## American de Tocquevilles

Alex Inkeles and Raymond A. Bauer

*The Soviet Citizen: Daily Life in a Totalitarian Society.* Cambridge, Mass.: Harvard University Press, 1959. Pp. xx + 533. \$10.00.

Reviewed by NICHOLAS HOBBS

CP's readers ought by now to be acquainted with Dr. Hobbs, Professor of Psychology in George Peabody College in Nashville. Merely in 1958 he reviewed for CP Symonds on psychotherapy (CP, Mar. 1958, 3, 53f.), Prescott on educating the child (Aug., 215-217), and Vexliard on hobos (Nov., 332f.).

IN 1831, Alexis de Tocqueville, a twenty-six-year-old assistant magistrate, was sent by the French government to study prisons in America. The book he wrote on return was a brilliant and wide-ranging treatise concerned with the workings of the new democratic republic, which he had examined with exacting individual scholarship. In the final paragraphs of Volume I of his *Democracy in America*, published 125 years ago, are some fascinating sentences, as pertinent today as then:

There are at the present time two great nations in the world, which started from different points, but seem to tend toward the same end. I allude to the Russians and the Americans. . . . The Anglo-American relies upon personal interest to accomplish his ends and gives free scope to the unguided strength and common sense of the people; the Russian centers all the authority of society in a single arm. The principal instrument of the former is freedom; of the latter, servitude. Their starting-point is different and their courses are

not the same; yet each of them seems marked out by the will of heaven to sway the destinies of half the globe.

De Tocqueville's prescient analysis is confirmed in *The Soviet Citizen*, an impressive product of contemporary collective scholarship, dedicated, tacitly in the present instance, to the proposition that the will of heaven is likely to be on the side with the most knowledge of the other. Inkeles, a sociologist, and Bauer, a social psychologist, had central roles in the Harvard Project on the Soviet Social System, a special study conducted by the Russian Research Center at Harvard for the United States Air Force. The project, which has yielded three previous books, took astute advantage of the presence in Western Europe in 1950-51 of more than a quarter million former Soviet citizens who escaped repatriation after the war.

Since they represented all walks of life, all ages, occupations, and nationalities, it was possible to draw from their midst a sufficient number of informants to permit us to study a great variety of problems concerning life in the Soviet Union. It is material from 764 long interviews, and detailed questionnaires from almost 3,000 different respondents woven together with relevant facts drawn from official Soviet sources and traditional Western scholarly studies of Soviet society, which constitute the main source on which this book is based.

The reader moves from the promising title of the book to knowledge of its primary source of data with a let-down feeling. He then proceeds warily, sleuthing for unwarranted extrapolations. If there is anything that a refugee is not it is a citizen. The authors might have entitled their book *The Soviet Refugee* and then let the reader delight in the discovery that he is in fact learning a lot about daily life in the U.S.S.R. But the title chosen can be defended both with regard to its substantive accuracy (the basic data were augmented and cross-checked by reference to other sources and by first-hand observations on visits to Russia in 1956 and 1957; the procedures and conclusions were subjected to critical scrutiny by knowledgeable colleagues in the Institute) and to its appropriateness from the standpoint of methodology. The authors are thoroughly sophisticated social scientists; they are meticulous in describing their procedures and careful to estimate the confidence that can be put in each generalization. Russia is fortunate in having available such a thorough and objective description of its citizens. One can fancy how helpful it would be to have from Russian scientists a counterpart of comparable scholarly quality entitled *The American Citizen*.

SINCE the authors let pass no unclear sentence and since their findings are so important, it seems appropriate to de-



RAYMOND A. BAUER



vote much of this review to quotations from the brilliant chapter entitled *The Future of Soviet Society*.

Whatever hopes may linger from 'X's' 1947 prediction in *Foreign Affairs* that the Soviet System could not survive a change in leadership are quickly dispelled:

Most students of Soviet Affairs . . . accept the idea that the system will not soon collapse from internal pressures.

With this wistful notion aside, the reader can reflect on the book's striking central thesis that the United States and Russia are in many ways much alike.

Perhaps the most important general conclusion which emerges from our study is that in large measure the response of Soviet citizens to their social system is to an extraordinary degree comparable to the response of citizens in other large-scale industrial societies, notably the United States, to their industrial system. Thus we have observed that the patterning of values about the occupational structure, of opportunities for mobility, of the evaluation of education, of ideas about child rearing, of communications behavior, and many other realms of experience is broadly similar in the Soviet Union and other large scale industrial societies.

Differences lie largely in the political realm and while these are profound they do not suggest any necessary weakness in the Soviet system. Even among a group of people expelled from the country, many of whom had been subject to 'the terror' and arrest, there was no basic questioning of the system, no commitment to what we regard as the essential political rights of man.

The execution of the program, rather than the conception itself, was deemed bad . . . Good rulers, kind, considerate, and compassionate, who 'cared' for people, and did not terrorize them or push them too hard, would be quite acceptable,

especially if they provided an increasing standard of living and opportunities for personal advancement.

And here are some concluding sentences from the book:

In the balance hangs the decision as to what the dominant cultural and political forms of human endeavor will be for the remainder of this century and perhaps beyond. It is perhaps only a little thing that separates the Soviet world from the West—freedom . . . We must look for our defense to the capacity of our own social order to yield fuller, richer, more dignified life under freedom not only for ourselves, but for the uncommitted, the half committed, the neutralists, and even those who have already cast their lot with the Soviet Union. If we are not equal to the task, we will leave it to the Soviet Union to set the pattern of human existence for the next half-century. Many of the distinctive features of Soviet life we have sketched in our book may then come to characterize not merely the Soviet citizen, but the citizens of the world.

How reminiscent of de Tocqueville!



ALEX INKELES



*There is much more that one might know than any of us are ever going to know. . . . This has nothing to do with the trivial fact that we don't work hard enough; nor . . . that things are difficult to learn. It is, rather, that any form of knowledge really precludes other forms; that any serious study of one thing cuts out some other part of your life. Narrowness is not an accident . . . but a condition of knowledge. . . . In all scientific things, . . . knowledge precludes other knowledge—by the very techniques, powers, and facts of its acquisition and by the way it organizes the chaos that is the world around us.*

—J. ROBERT OPPENHEIMER

BEACON

HOW  
DOES FICTION  
FEED  
THE PSYCHE?

FICTION AND THE  
UNCONSCIOUS

by SIMON O. LESSER

Not satisfied with his own experiences, man demands the *formed* experience of fiction . . . to soothe guilt, allay fears, fulfill and enrich desires. It is this intricate process of response that Lesser describes. Scientifically, and with precise psychoanalytic detail, he cites the specific characteristics of fiction that feed the various aspects of the human psyche: appetites, concrete perceptions, and moral judgments . . . on both the conscious and unconscious levels. Included in the book are studies of HAMLET, THE IDIOT, OEDIPUS, OTHELLO, MOBY DICK, SONS AND LOVERS, and an Appendix, "A Note on the Use of Scientific Psychological Knowledge in Literary Study."

" . . . the most intelligent, full length study I have seen of the psychological hold of all forms of storytelling on the mind of man."

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— Dr. Leonard Manheim

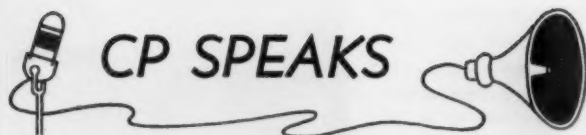
"His book is the rare instance of psychological and literary sensibilities enhancing each other . . . advances the psychological interpretation of literature from the paradigm of wish-fulfillment and unconscious aim to the more inclusive approach of modern psychoanalytic ego psychology."

— Contemporary Psychology

" . . . it is remarkable that we have had to wait so long for a comprehensive study . . . such as Mr. Lesser has provided in this fascinating book."

— Ernest Jones

Beacon Press, 25 Beacon St., Boston, Mass.



## HOW MANY BOOKS ARE GOOD?

ONE meets casually in the informal conversation of psychologists two seemingly incompatible complaints about book reviewing. These are they.

(1) Most book reviewing is favorable and, when one has regard to the poor quality of the majority of the books published, one can only conclude that a bias toward approbation is operating. Why should there be? There is one minority group with an answer. It avers that the excess of praise over dispraise is due to lack of courage in the reviewers and recommends that all reviews should be anonymous so that reviewers can speak without fear (cf. *CP*, June 1957, 2, 164f.).

(2) Book reviewing is irresponsibly harsh and brutal, an outlet for latent aggression, where criticism, in Oscar Wilde's phrase, does indeed become a form of autobiography. In not many other areas of learned literature is it permissible so easily to impugn a writer's integrity and belittle his intelligence, if not by direct insult, at least by sly but unmistakable innuendo. Remove the police and mayhem is free. Are the book-review columns unpoliced?

Most reviews are indeed favorable. That is a fact. Edith Annin found that of 295 reviews in four journals (not including *CP*) 240 were favorable in some degree and only 55 unfavorable (*CP*, Aug. 1958, 3, 303). Is this a measure of the reviewers' cowardice or is it perhaps a consequence of successive criticism? There must surely have been progressive selection. The reviewed book had first to pass the author's superego, then the publisher's judgment with his thought about what readers will approve and buy, then an editor's decision in choosing it for review, and only finally the reviewer's appraisal after the three previous evaluations. If the world were

to end with four successive Judgment Days (like getting a PhD), there would surely be more sheep than goats on the last day (as it is with PhDs). Successive selection works. The excess of favorable reviews would not be indicating a bias toward approbation, would it?

The psychophysicist might, however, wish to pause at this point. He knows that 'absolute judgments' of a batch of stimuli or objects that vary in magnitude throughout a given range are judged relative to an 'average' magnitude of the item in the range. A subject can sort a set of heavy weights into groups of *Heavy*, *Medium*, and *Light* just as soon as he learns by experience at what magnitudes the range is placed, where its top and bottom, its 'anchor-points,' lie. And then he can get comparable results for a set of light weights, even though the light weight called *Heavy* is much lighter than the heavy weight called *Light*. There'll be as many *heavies* as *lights*, more or less, wherever the range is placed, or at least the *heavies* and *lights* will divide at some average, like the geometrical mean. Why is it not the same for books? Why does not the generic book reviewer, that composite of all book reviewers, find as many good books as poor, dividing them on either side of the mean of all books? Isn't he finding too many good books, if he gets more good than poor? Mustn't we assume that he is biased toward approbation?

Perhaps he is in a way. Perhaps the book reviewer thinks he knows what books are like, where the top anchor-point lies, the splendid book that occasionally though rarely actually turns up for review, and also where the bottom anchor is, the book so bad that it never gets far enough along to ask for review. If he takes his range from his general experience and not from the

books actually in hand for reviewing, he would get more goods than bads. He keeps his bottom anchor quite low, where he thinks he knows it belongs—this is a plausible hypothesis, suggested to *CP* by Dr. R. J. Herrnstein.

Now what has become of the hypothesis that most reviews are nasty, that they release latent aggression? As far as the *most* goes, the view is just plain wrong. The ugly reviews make news, as does all aggression, and thus they make the headlines of cognition. They get the most attention; they are talked about most; they are remembered longest; and they hurt you most if you yourself are the *homo* that the criticism is *ad*. But they do not seem to be the more numerous. Edith Annin did not find them many. *CP*'s experience is no good guide because its not always successful attempt to interdict *ad hominem* criticism in reviews is getting to be understood and accepted. Abuse in *CP* nowadays is apt to come from the peripheral reviewer, who does not regularly read *CP*, who is perhaps not a psychologist, or from the letter-writer who has no previous instruction.

That there is latent aggression, all ready to explode when pride is injured and the rules are relaxed, *CP* knows well. It occurs now for *CP* most often in the first drafts of letters of dissent, for these are written to *CP* without the prior demand to substitute the *ad verbum* for the *ad hominem*. *CP* sends these letters back and begs their writers to play this tennis for riposte to rejoinder, back and forth, at a mature intellectual level, where ideas are the targets and those who spawned them are ignored—ignored for no better reason than that sound judgment evaporates when egos are impaled.

In *CP*'s infancy democratic readers, fearing autocratic censorship by *CP* and noting how much longer is the list of *Books Received* than the tale of books reviewed, worried lest *CP* be skipping books. *CP*'s reply was: Name a book that should have been reviewed and was not. No one did. Plenty of books that could have been reviewed and were not, but none that should. *CP* was getting democratic coverage. Now the opposite anxiety is beginning to emerge, as perhaps it always does when liberals wake

up and find that democracy leads toward mediocrity: Why, it is asked, does *CP* review so much "trash"? Well, *CP* ought to review some 'trash,' briefly and succinctly, as a matter of professional discipline. An author ought not to feel that the worst that can happen to him is to be ignored. There must be stern judgment for a discreet sampling of the poor books, especially when the reviewer perceives a book so in error that it will be doing harm and when an eminent author lends his prestige to strengthen what the reviewer sees as nonsense.

What then about that second quarter of the books, the poorest among the favorably reviewed? *CP* would have trouble spotting them before the painfully achieved reviews came in, yet might it alert its consultants and reviewers? Reject more, it could say. Review what you think are good and very good. Skip the mediocre. Review the poor ones unfavorably if you find a good reason for doing so; otherwise skip. And haul your bottom anchor up a little anyway to make the poorer books seem really poor. Would this work?

One trouble with that solution is that it prevents the eliminated mediocre books from participating in 'cybernetic justice.' There could be no Daniel come to judge the good book, stigmatized as mediocre, because no Daniel would ever know of its fate. Thus *CP* is found facing, in its small way, the ancient dilemma of well-intentioned autocracy vs. dull democracy. What should it try to do?

#### BOOKS TO COME

MORE than four years ago Murray Sidman, discussing William Verplanck's discussion of B. F. Skinner's unstatisticized and untheoreticized learning 'theory,' wrote in *CP* at its very birth (*CP*, Jan. 1956, 1, 8):

Skinner's criterion of acceptability of experimental data is not the confidence level of the statistics, but one's degree of confidence in the competence and integrity of the experimenter. The experimenter's, skill and integrity are maintained, in turn, not by his statistics, but by the traditional principles of replicability by others and of the consistency of data when they are ap-

plied to new situations and with new techniques. A requirement of significance at the .001 level is in no sense an adequate substitute for the more laborious but eventually self-corrective process of systematic replication.

That started discussion—of systematic self-corrective replication—among the Skinnerians and the other behavioral experimentalists, so Sidman wrote a paper about it. Written, the paper kept bursting its seams, so Sidman made it into a monograph, dealing with variability, data, experimental design, various kinds of replication, and lots of other things, still so many too many to stay put that Sidman then expanded his exploding idea into a book, which Basic Books will publish, maybe in September, just before it turns into a multi-volume handbook. In March its title was *The Evaluation of Experimental Data*. In April the title had become *Behavioral Research: Its Practice and Evaluation*. Now let him who knows how to extrapolate from two points specify the September title. *No Confidence in Confidence?*

#### OMNIUM-GATHERUM

T. ERNEST NEWLAND writes into say that *CP* erred when it said that "he got the program for education of exceptional children started in Pennsylvania" (*CP*, Feb. 1960, 5, 43) for the program for handicapped children had been going on nicely for 20 years before Newland arrived. What Newland did was to get the name changed from *handicapped* to *exceptional*, thus bringing in the exceptionally superior as well as the exceptionally inferior.

DR. THOMAS F. A. PLAUT said of Dr. E. Gartley Jaco's *Patients, Physicians and Illness: a Sourcebook in Behavioral Sciences and Medicine* (Free

Press, 1958; *CP*, Mar. 1960, 5, 96-98): "This sourcebook does justice to the broad range of investigation and speculation in a new interdisciplinary field." *CP* inadvertently reversed Plaut's meaning in editing, although it could have no motive for belittling Dr. Jaco. It thinks it wanted to say: "... nor does this sourcebook fail to do justice to the broad range of investigation and speculation in this new interdisciplinary field," and then—*horribile dictu*—it left out the words *fail to*! Yes, *CP* has read Freud's *Psychopathology of Everyday Life*, but just the same, when you are tired and spill soup on your shirt front, it isn't always because you want sympathy. There is such a thing as chance, and why should *CP* hate either Plaut or Jaco? But *CP* is sorry. It wants to be infallible. It does not expect to, but it wants to.

LONG ago *CP* tried to get up a list of the Psychological Editors-Consultants-Advisors that the various publishers have—those publishers who publish enough books on psychology to need special help. The plan did not work out. In some quarters the turnover of Advisors is too rapid. In others the publisher "protects" the Advisor by keeping him anonymous. One hydra-cerebrated publisher could not find out who his Advisor was, that is to say, one head did not know which other head to ask.

This confusion has never, however, afflicted Century, or D. Appleton-Century, or Appleton-Century-Crofts in their thirty-one-year psychological history with Elliott, Elliott-MacCorquodale, and now Elliott-MacCorquodale-Lindzey. *CP* announces that ACC has added Gardner Lindzey to its cathecting squad and should thus be well set for the next thirty-one years. May this interpersonal triad prosper!

—E. G. B.



The effect of using quotation marks with slang is merely to convert a mental into a moral weakness. When they are not used, we may mercifully assume that the writer does not know the difference between slang and good English, and sins in ignorance: when they are, he is telling us, I know it is naughty, but then it is nice. Most of us would rather be taken for knaves than fools; and so the quotation marks are usually there.

—H. W. and F. G. FOWLER

# Humanitarianism, Psychiatry, Social Science—an Alliance

Harry A. Wilmer

*Social Psychiatry in Action: A Therapeutic Community.* Springfield, Ill.: Charles C Thomas, 1958. Pp. xxiii + 373. \$8.75.

Reviewed by RICHARD H. YORK

*Dr. York is Chief Psychologist and Director of Research in the Butler Health Center in Providence where he is engaged in studying a community psychiatric center for social treatment of the kind discussed in the book under review and also in his own earlier collaboration with Greenblatt, Brown, and Hyde, From Custodial to Therapeutic Patient Care in Mental Hospitals: Explorations in Social Treatment (Russell Sage Found., 1955; CP, Jan. 1957, 2, 7f.). He has been in this work for a decade, with Dr. Robert W. Hyde and at the Boston Psychopathic Hospital.*

A WIDE variety of concepts, practices, and settings for psychiatric treatment have now been associated with the phrase "therapeutic community." Wilmer's experiment in social treatment at the Oakland (California) Navy Hospital in 1955 adds still more variation to the total picture, thus providing valuable data for the closer study of social psychiatry. This work is of more general significance, however, as a thoughtfully conceived, operationally cohesive endeavor. Concepts, procedures, and behavioral descriptions are tightly interwoven in an absorbing account of how patients and staff explored and met their problems in getting along together.

Dr. Wilmer's determination to undertake such a project stemmed from his humane revulsion against the neglect and brutality he had observed in several hospitals and his contrasting observations of several English therapeutic communities (Jones, 1953; Reese, 1955). Previous experience as a TB patient, group therapist, and analyst also contributed to his able management of the

project. He was administrator and therapist on a 37-bed Navy admission ward for ten months. These patients were experiencing their first breakdown and had been unselected as to their type of psychiatric disorder. Frequently they arrived on the ward in restraints, highly medicated. Each patient's stay on the ward was predetermined by Wilmer to be ten days before transfer elsewhere, and there was also a frequent rotation of the staff.

At first, these conditions appear to be rather unlikely for the establishment of stable social norms and helpful intimate relationships. Wilmer, however, made maximum use of the Navy's traditions of orderliness, respect for authority, and group loyalty. In addition, he established several unequivocal, humane expectations for the behavior of both staff and patients. He built up, and was himself consistently involved in, many formal and informal procedures which tended to evoke adherence to these standards. The disorganized, fearful, and alienated patients were in this way successfully integrated into the ward community without the use of seclusion or restraint, and with a minimal use of medications. While Wilmer properly does not assume cure for these patients, he does assume and generally documents their partial social recovery.

THERE is a basic aspect of methodology which may account for much of the success of this and comparable programs of treatment, an aspect that has not been emphasized enough in the literature because members of communities which are therapeutic tend to take that fact for granted whereas visitors or

professional outsiders are not usually in a position to perceive it. Wilmer says that he considered his patients to be "normal people who are sick." This is a first premise which redefines the whole social situation in treatment as compared with traditional psychiatric practice. Stemming from this premise are specific assumptions which are at once the leader's expectation for the community members, a definition of abilities characteristic of the patients and staff, and the norms for social behavior on the unit. It is assumed that even the most disturbed patient is able to express his tensions and fears, to work at understanding them through sharing the basis of his experiences with others, to maintain self-control, and to conform to social conventions. Stated in the simplest terms, the patient does not so much obtain these abilities as an effect of treatment as he contributes them to the treatment.

The dynamic heart of the therapeutic community lies in an initial and continual emphasis on the 'well' or ego part of the patient accompanied by a constant "concern that the dignity and self-respect of the patient never be violated." Investigation of the deeply troubled 'sick' part of the patient goes on, but it takes place in a context where forms of social action maximize the chance that the patient's given capacities will be exercised or mobilized. The relatively successful moral treatment hospitals of the middle 1800s (Bockoven, 1956) were founded on the same premise about the character of the mental patient. After their decline, in part due to inadequate scientific foundations, this principle re-emerged most clearly in the early work of H. S. Sullivan (1932, 1947), who held that anyone in an interpersonal field experiences, more or less, the tensions therein, has all the primary data there is to analyze, and may thereby influence the course of events.

What is new on the current scene and well represented in Wilmer's work is the increasing fund of knowledge of social science and of specific techniques of therapy which are available to support, refine, and systematize the humanitarian premise and actions. His foremost formal technique was a daily ward meet-



ing for the group therapy of all the patients and staff. Half of this book consists of detailed reporting and analysis of what transpired in these community meetings. Wilmer consistently weaves back and forth from interaction process to interpretation, to process again, indicating how subsequent events confirmed or negated his own and the patient's interpretations. As the group explore their situations with each other, the relationship of such severe individual symptoms as delusions to the ongoing interaction process is seldom obscure and is frequently convincing.

Two additional characteristics of this and similar communities are apparent in the daily group meeting, the staff meeting which regularly followed it, and in many other meetings and procedures. These occasions assure rather complete availability of the members to each other, maximizing the use of interpersonal resources. They also assure fairly full information about what is actually going on. We have here a critical factor in that it provides the basis for more accurate assessment of the meaning of events or any selected aspect of behavior.

THERE are other important contributions in this work. For instance, there is a discussion of the meaning of silence and laughter in group meetings, a systematic presentation of the conservative position on the use of drugs, and a very sensible discussion about combining the roles of the administrator and the therapist in the same person.

The presentation shows an over-all high order of internal consistency in its data, in the logical sequence of events as presented, and in the various steps taken to assure reliability of information and interpretation. While there is in the present report no pretense at systematic evaluation of results of the operation, still the lack of some minimum or gross formal assessment is the major shortcoming of the report. Such an evaluation may yet be forthcoming, for the author mentions a further report is in press (Naval Medical Research Institute). We may hope that it will contain an estimate of the degree of social recovery of the hundreds of patients who were admitted to this ward in psychotic

states. We are probably entering a period where a combination of studies will indicate what the future contribution will be of this current alliance of humanitarianism, psychiatry, and social science as represented in therapeutic communities.

This book should be instructive and rewarding for any student of human behavior. It supports the growing evidence that 'crazy' symptoms and disorganized behavior, like delusions, have an ob-

servable, definable relationship to real events in the past and present social environment of the individual.

The thesis that behavior is a function of the individual and his environment is certainly not new to social and biological science. Should the 'special case' of mental illness, which has for so long been regarded as an exception, turn out to prove the general thesis, then we should indeed be entering a new era in many fields of study and practice.

## Whither Psychoanalysis?

Jules H. Masserman (Ed.)

*Science and Psychoanalysis*. Vol. II: *Individual and Familial Dynamics*. New York: Grune & Stratton, 1959. Pp. vi + 218. \$6.75.

Reviewed by FRED J. GOLDSTEIN

*Dr. Goldstein is Chief of the Department of Clinical Psychology in the Los Angeles Psychiatric Service and teaches clinical psychology in the University of Southern California. He has long been deeply interested in the nature of psychoanalysis, what it is and what it could be, having been stimulated to rigorous thinking by Gardner Murphy and later by Egon Brunswik. Recently he reviewed Milton Kline's Freud and Hypnosis in CP (Sept. 1959, 4, 280f.).*

THIS book is the second volume published by the Academy of Psychoanalysis in the series entitled *Science and Psychoanalysis*. It is composed of two separate symposia; the first is a symposium on masochism, and the second a symposium on familial and social dynamics. Like the first volume in the series reviewed a few months ago (*CP*, Sept. 1959, 4, 273f.), this volume again comprises largely clinical considerations which do not justify the strict use of the word *science* as it is typically applied by our 'tough-minded' psychologists. It would be too easy to cavil about experimental controls and other more usual considerations in 'science.' However, shooting at ducks in a bathtub hardly seems sportsmanlike, and at

a gross empirical level there is much of value here. A more positive direction for this reviewer would be the evaluation of these symposia as indicating a trend exemplified in psychoanalysis and psychotherapy.

In considering the steps from empirical-descriptive levels of knowledge to causal-explanatory levels, we find psychoanalytic theorizing on masochism demonstrating frequent leaps from description to explanation without bridging the gap. The consideration of masochism raises problems of definition and consistency of usage that cause continuity and consistency of treatment to founder on a semantic reef. Is masochism a symptom? Are these people a separate diagnostic entity? Does masochism fit into certain personality structures only? Can we explain before we define? These questions raised by several participants indicate the lack of semantic specificity prevailing in psychoanalytic language. The noise level is high, and the kind of formalistic definition of biology by a Woodger or Mainx would probably have a salutary effect in clinical theory; it would develop objectivity. One need only to list the varying usages for the term *masochist* for the neo-Freudian, Adaptation-

alist (Rado), and Freudian to note how lacking common meaning is. Similarly, the right treatment for the 'masochist' seems equally varied, so that there is no royal road to success for his treatment. The psychological reader would probably find Masserman's paper the most rewarding in this first section of the book. He repeats the fundamental question as to whether the stigma of masochism is a therapist's designation, reflecting a certain cultural bias and system of values. Psychological readers will probably recognize an almost literal synonymy between the untruncated version of the Law of Effect (Thorndike) and Rado's principle of "negative hedonic self-regulation" used to describe the masochist.

One might well conclude that while many raise the flag of understanding in the realm of masochism, none can claim to have surveyed and studied the area sufficiently. The need for tying down the descriptive categorization continues to be a pressing problem; when one uses the term *masochism* we do not yet mean one phenomenon. Many areas of psychoanalytic theory demonstrate a similar ambiguity.

THE second part of this book shows a marked shift from the classic psychoanalytic model of the couch and the basic rule. The shift, as this reviewer sees it, in the conjoint or family type of therapies, is against the belief that the royal road to all reality can come from any single aspect of the total setting. The omnipotent and omniscient fantasy of the psychoanalyst is giving way to one that can only have a salubrious effect on him and on the society that has to deal with him.

A fundamental question raised in this symposium is whether the formulation of the analyst about the nature of the parents and their interaction with his patient can be established from the restricted single source of information, viz. the patient. Psychologists will recall the Frank Beach article, *The Snark was a Boojum*, where a similar question was raised about the field of learning being the learning of the Norwegian Rat. The formulation of Ackerman, Spiegel, and Jackson suggests that the picture developed thus far introduces

considerable distortion of 'reality.' When we consider that such data in an adult are extrapolated back to infancy, and further that one focus of the oedipal triangle is used to construct the other two vertices, the challenge to review material from the family and societal focus seems clearly posed.

CORRECTIVE changes within the total field of psychotherapy are indicated in the existential analytic movement as well as here. The statement by M. Boss at Barcelona in 1958 offers the keynote here as well: "Not without Freud, but not with Freud alone." Here analysts honor Freud as Jackson would, by building from his beginnings, rather than by repeating dogma unswervingly.

In this symposium, there are the orthodox who always start and end with an affirmation to father of fealty. However, several of the groups working in family dynamics speak a new language, present a new vocabulary, and show a more rigorous, sophisticated approach to research. Ackerman boldly speaks of the inadequacies of Freudian theory regarding the family. The protean press into the psychoanalytic mold is not strong in this group of papers. Ackerman notes that the royal road to knowing the social environment that made the patient ill is through knowing the family at first hand. The ritual avoidance of members of the patient's family is increasingly questioned. Interdisciplinary research groups demonstrate that where the human psychotherapist is nondefensive, broadened knowledge and outlook result for all concerned. The "therapeutic community," the participant observer, and the contributions of anthropologists to social class and patient prognosis, all indicate a direction of growth in this field. If what is indicated here casts the future's shadow, then we can see in joint effort a move towards greater fusion of "science and psychoanalysis" that can only lead to fruitful issue. The psychological reader might find such a fusion exemplified in Spiegel's article, where schema of "universal" value orientations of Florence Kluckhohn is used to view transference and countertransference. Value orientations are guides to action without conscious awareness, and mesh nicely with many phenomena that

are usually attributed to transference and countertransference.

In general, it seems that the ferment and struggle to direct the clinical data towards pragmatic ends demands a greater knowledge and more objective test and verification than is readily accepted by the psychotherapist. This struggle to expand knowledge and increase its effectiveness should lead ultimately to the pragmatic goals that would follow from the marriage of science and psychoanalysis. The direction seen in this book is promising. Those persons interested in the clinical account of masochism or in the divergent perspectives found in a new field of psychotherapeutic treatment and research will find that reading some of these papers is a stimulating and thought-provoking experience.

## Technicways vs. Folkways

Paul H. Landis

*Social Problems: In Nation and World.* Philadelphia: J. B. Lippincott, 1959. Pp. xvi + 752.

Reviewed by BASIL S. GEORGOPOULOS

who is a Lecturer in Psychology and Sociology at the University of Michigan and Study Director of its Institute for Social Research. He is a Michigan PhD in social psychology with earlier training in sociology at the University of California at Los Angeles. For the last five years he has been a member of the research staff of the Organizational Behavior Program of the Institute for Social Research.

TRADITIONALLY textbooks on social problems have tended to be provincial, with heavy emphasis on the home front and relatively little systematic effort toward comparative understanding or cross-cultural generalization. As industrialization and cultural contacts spread, there is now an increasing recognition that many of the social prob-

lems encountered in Western societies are also familiar and troublesome in other parts of the world. This change has stimulated a new interest in studying social problems within this broader context. *Social Problems in Nation and World* constitutes a welcome departure from more conventional approaches, like Jessie Bernard's *Social Problems at Mid-century* (1957). The former, however, represents a more extensive and more ambitious effort.

Dr. Landis, who is professor of sociology at Washington State University, has had a background particularly suitable to such a task as he undertook in his present work. In the past decade he has written several books in such related areas as social control, population problems, adolescence and youth, marriage, and social policies. In *Social Problems in Nation and World* he has attempted, with a good measure of success, a synthesis of a large volume of historical and contemporary observations and data about social problems in our society and in other nations. He has covered a wide range of subjects—from crime and delinquency, alcoholism and addiction, prostitution and sex offences, through mental illness and neurosis, to child training, family, population problems, and even the problems of conservation of natural resources, of war and peace, and of social planning and policy.

This diversity of content is in part a function of the author's broad conception of social problems as "man's unfulfilled aspirations for welfare." The conception and the consequent variety of problems examined give the book unusual breadth, but, at the same time, they are responsible for a certain lack of depth, of methodological rigor, and of consistent theoretical tightness. The author's analysis and interpretation follow primarily sociological lines, but he does not overlook the role and implications of psychological (also economic and political) forces associated with the various problems. Psychologists, as well as others, who take a more monistic position in attempting to explain and understand problems will find the present book instructive.

The book makes a good case for the proposition that increasing industrialization, urbanization, and scientific and

technological development, if accompanied by education, technological leadership, and proper social planning and policy, can provide the answer to mankind's ills. These ills, manifesting themselves as failures in personal or personal-institutional adjustments and deficiencies of social structure, as "folkways" making room for "technicways," can be seen as products or concomitants of the same basic forces: the spread of industrialization, urbanization, and technology resulting in clashes between different value systems, the increasing population mobility, and the shifts from primary to secondary group experiences which necessitate social reorganization

and personal readjustment. Landis discusses the anxieties, shocks, stresses, and strains experienced by people under the powerful impact of these forces and documents his statements, although he does not treat the mechanisms and dynamics of psychological adjustment as adequately as most psychologists would like.

On balance, this book constitutes a worthwhile contribution to the literature on social problems. It reads well, is well written. Students will not find it a difficult text, despite its size, and nonprofessional persons interested in social problems will not find it overtechnical.

## Almost Everything about Studying Social Relations

Claire Selltitz, Marie Jahoda, Morton Deutsch, and Stuart W. Cook

*Research Methods in Social Relations.* (Rev. 1-vol. ed.) New York: Henry Holt, 1959. Pp. xviii + 622. \$5.50.

Reviewed by JOHN T. LANZETTA

*Dr. Lanzetta is Professor of Psychology at the University of Delaware and Director of the Fels Group Dynamics Center there. He is concerned with nature of leadership and related problems of group dynamics.*

THE debate still rages, outside the circles of the social sciences, as to the scientific status of social psychology. It is difficult enough for some of our scientific brethren to concede the respectability of the methods used in comparative and sensory psychology and in the psychology of learning. For them to admit that scientific procedures are also capable of being applied to problems of social relationships is tantamount to divesting the concept 'scientific method' of any meaning.

Either blithely unaware of their suspect status, or possibly in response to the challenge, social scientists have in recent years devoted considerable effort to codifying their 'scientific' approaches

to problems of social psychology. The early years of feverish exploration and rapidly changing techniques in empirical investigations of social relationships provided little opportunity for systematic development of methodology, and codification and communication seemed somehow premature. Gradually the enthusiasm of the explorers has now given way to the sober and systematic efforts of the later homesteaders. Solidifying the gains made has become as important a goal as extensions of insights and hypotheses into new domains. Methods appropriate to the careful and systematic establishment of reliable empirical relationships have been rapidly developed and are gradually being diffused from the front lines of research to the classroom via general texts on methods and specialist books on specific techniques.

An early entry into the field of basic, broad-gauge books on methods was the two-volume text by Jahoda, Deutsch,

and Cook (Dryden Press, 1951). Prepared under the auspices of the Society for the Psychological Study of Social Issues (SPSSI), the volumes attempted "to bring together on an introductory level the considerations which enter into every step of the research process." Part I, *Basic Processes*, was produced by the three authors and covered the major steps of a scientific inquiry into social relations, from the selection and formulation of the problem through analysis, interpretation, and presentation of the research results. Part II, *Selected Techniques*, was a collection of contributions by experts which dealt in more technical detail with specific methodological problems such as content analysis, observational field-work methods, and scaling techniques. Throughout, the presentation had a special emphasis arising from the authors' convictions that research should contribute to the solution of practical problems. Thus problems of collaboration between social scientists and the users of research were treated in some detail, and illustrative material was drawn from the area of research on prejudice, a pressing social problem where collaboration with social agencies was particularly important. The text filled an important gap and was widely adapted for use in graduate and undergraduate courses.

NINE years and hundreds of studies later, a revision of *Research Methods in Social Relations* has appeared. In revising, the authors had "two main goals: to bring the book up to date, and to organize it as a one-volume text suitable for use in undergraduate courses as well as introductory graduate courses in research methods in social psychology and sociology." For the new edition, the authors of the earlier work were joined by Claire Sellitz who assumed major responsibility for the revising, in close cooperation with the original authors.

Miss Sellitz is a research associate at the Research Center for Human Relations of New York University, which, at the time of the earlier edition, was also the home of Drs. Jahoda, Deutsch, and Cook. Dr. Cook remains at NYU, as head of the Department of Psychology and director of the Center for Hu-

man Relations, while Drs. Jahoda and Deutsch have assumed new positions. The former is at present senior lecturer in psychology at Brunel College of Technology, London, while Dr. Deutsch has joined the staff of the Department of Communications and Social Science Research of Bell Telephone Laboratories. The authors bring to their task a wealth and variety of research experience obtained in academic and applied settings. All are active contributors to the accumulating body of research in social science and individually and collaboratively have made outstanding contributions to the research literature on intergroup relations and small group behavior. Their sophistication and grasp of the concrete realities of conducting research in a field setting are evident throughout the volume.

The general aim of the revision, like the original, is to present a comprehensive picture of the research process—from formulating the problem to the final report. Although the steps are presented in a logical sequence, the authors take pains to stress that the phases are neither clearly discriminable nor independent. Coverage of the various research activities and their implications is excellent, although the text obviously does not aspire to be a comprehensive handbook of specific techniques. Rather the intent is to provide a description and evaluation of a wide variety of techniques and procedures with the student being directed to other sources for details. The selection of these additional references is unusually up to date (articles appearing as late as 1958) and judiciously selective.

The audience is both the undergraduate and graduate major in sociology and social psychology and the user of social-science research. For the latter, it is hoped that exposure to the potentialities and limitations of research in this area will prevent the development of unrealistic expectations, with the eventual disillusionment and accompanying negative reaction. Experience with the earlier edition indicated that certain complications of style and some highly technical sections made for difficult reading. In the revision the material in Volume I is clarified by simplification of the language and expansion of the

discussion, while much of the technical content of Volume II is eliminated. The result is a one-volume text of relatively uniform difficulty which retains the rigor and excellence of the earlier edition.

APPROXIMATELY half of the book is devoted to general problems of measurement, analysis, and interpretation, and to discussions of the specific techniques of collecting data. Problems of reliability and validity are stressed, along with cogent recommendations for assessing the adequacy of an instrument or technique with respect to these criteria. The authors do not shirk the onerous task of indicating the shakiness of the methodological foundations of much of our research in social science based, as it often is, on instruments of low reliability and doubtful validity.

The techniques of the collection of data as discussed include those typically associated with social-science research: observational methods, questionnaires and interviews, the use of documents, records, and census material. In addition, the authors present a careful, comprehensive, and critical survey of projective and other indirect methods, providing a link between the methodology of personality assessment and techniques for the measurement of social attitudes. Throughout these sections there is evident an appreciation for basic methodological issues as well as for the practical, concrete problems involved in constructing and utilizing an instrument. Aspects often taken for granted such as the establishment of categories, coding, and tabulation are treated in enough detail to provide guidance and proper orientation to the uninitiated.

The sections on research design, the logic of testing hypotheses, and problems of causal inference are clear, concise, and rigorous—as good a presentation on the elementary level as is available and certainly within the grasp of most advanced undergraduate students. One of the nicest features is the treatment of the issues involved in selecting and formulating a research problem. The necessity for careful formal and working definitions of concepts and the intimate relationship between such definitions and the potentialities for relat-



ing the research to other studies and existing social science theory is indicated.

The authors have a happy facility for selecting meaningful and appropriate research studies for illustrative purposes. Often examples drawn from published studies present a misleading picture of the research process. False starts, conceptual ambiguities, subtle factors entering into the development of operational definitions, are filtered out in writing for publication. The research emerges as a logically consistent, well-articulated whole, conforming to the stereotyped model that has developed in the scientific as well as in the public mind. The actual research process is, of course, "a rather informal, often illogical and sometimes messy looking affair," and includes "a great deal of floundering around in the empirical world." Some sense of this 'floundering' ought to be conveyed in illustrative material but it is difficult to reconstruct the process from published reports.

The authors avoid this pitfall, in part, by relying primarily on research that is familiar to them personally. They are thus able to direct attention to the intervening steps between the vague conception of a research problem and its final emergence as a carefully designed and executed study. The process still looks cleaner than it usually is, but some of the flavor of the struggle is communicated. The examples are excellent in one other respect. They often involve problems of social significance of the type which have traditionally been 'solved' by debate. Demonstrating that a scientific approach to such issues is feasible may facilitate the student's commitment to a rational approach to the treatment of social problems.

CONSISTENT with the general emphasis on concrete and practical considerations as well as basic methodological issues, the authors include chapters on the research report, applications of social research, and the relationships between research and theory. In three appendices they also cover the estimation of time and personnel requirements for a study, an introduction to sampling theory, and procedures in questionnaire construction and interview techniques. Once again

the discussions are comprehensive and detailed enough to serve as excellent introductions to these aspects of the research enterprise. Several of the sections, in fact, contain experienced observations that are difficult to obtain from other sources and are worth the attention of the professional as well as the student.

The one surprising and disappointing feature of the book, considering its extensive coverage, is the relative lack of attention directed to laboratory experiments. Nothing comparable to the material covered in Festinger's excellent chapter in *Research Methods in the Behavioral Sciences* (Dryden Press, 1953) is included, and few, if any, illustrative examples are based on laboratory studies. Problems of simulation, selection of tasks, manipulation of experimental variables, objective recording and instrumentation, subject recruitment, post-experimental catharsis (when necessary), and ethical issues in laboratory studies are only incidentally touched upon, or completely ignored. True, the orientation of the book is to research which has potential application to problems arising in the contemporary world and, traditionally, field research has been viewed as offering greater possibility of direct application. But, increasingly, social scientists have been turning to the laboratory to find the answers to practical problems (sometimes utilizing highly realistic simulation; e.g., the System Development Group at Rand Corporation) and to refine and develop their theories. If Lewin's adage that there is nothing so practical as a good theory has any validity, then the potential of laboratory investigations for the solution of practical problems has been grossly underestimated.

Whatever the status of laboratory studies with respect to 'real world' issues, they are an important and growing source of the data upon which theory in social science is based. A text on research methods ought thus to provide some account of specific methodological problems encountered in such research. For this reviewer, a confirmed laboratory man, this inadequacy limits the usefulness of what is otherwise an excellent presentation of the methods of research in social science.

## Fundamentals of Guidance in Schools

Donald G. Mortensen and Allen M. Schmuller

*Guidance in Today's Schools.* New York: John Wiley & Sons, 1959. Pp. viii + 436. \$5.75.

Reviewed by KENNETH B. HOYT

who is Associate Professor in the College of Education at the State University of Iowa. He teaches courses in counseling and guidance, is major advisor to graduate students preparing to become counselors, and was recently chairman of an APA committee on the preparation of school counselors.

THIS book, written by two men who work in institutions primarily concerned with teacher education, is intended to be used in introductory courses in guidance for teachers, school administrators, and those beginning a secondary-school counselor preparation program. The authors, both of whom received their doctorates from the University of Southern California, have chosen to depart rather radically from typical books in this area in approaching the topic of guidance in today's schools. Instead of devoting major content to a description of guidance services and the ways in which they are implemented in school settings, they have chosen to describe guidance in terms of basic notions inherent in the concept of guidance utilizing several areas of psychology, education, sociology, and economics. They refer to these notions as the "theoretical structure of guidance." To talk about the theoretical structure of guidance makes about as much sense, however, as to talk about the theoretical structure of applied psychology. Guidance, as it is practiced, represents applied psychology squared. Guidance has its roots in several disciplines, each of which does have a respectable theoretical structure, but why start to talk about the theoretical structure of a field

which, in and of itself, has never had such a structure?

Psychologists will be interested in the authors' attempts to present basic features in such topics as the need concept, the role of emotion in personality development, the defense mechanisms, the concept of maturity, adjustment, and the Oedipus complex, each in a page or two. These are but a few examples of the many topics drawn from psychology, one of the four major areas in which the authors find a theoretical structure for guidance.

There are two essential questions which must be asked here. The first—is the material presented essentially accurate?—can be answered rather easily in the affirmative. The authors have done an excellent job of describing several rather difficult concepts in a relatively few, well-chosen words. Their writing is clear and easily understandable throughout the book.

The second question—is this material relevant for its intended audience?—is not so easily answered. One could argue that elementary- and secondary-school classroom teachers (who are the biggest group of intended readers of the book), being ignorant of most areas in psychology, would find the material in this book new, interesting, and helpful. For these teachers, this book might serve to provide a perspective of the basic roots of the guidance movement, one which would aid their understanding of the process of guidance as it is practiced in schools. Students beginning a graduate program with a major in School Counseling and Guidance might be expected to find in the contents of this book a rationale for much of the structure of their required sequence of courses. In this sense they, too, would find this approach helpful.

On the other hand, one might also question this emphasis for both the teachers and the prospective school counselors. For one thing the book provides too few concrete references to the role of the teacher in the guidance program. It would be difficult for the teacher to clearly perceive, on the basis of this book's content, how she can or should contribute to the guidance program in the school. There is also the danger of undereducating the reader by

making the descriptions of difficult concepts, none of which can be fully explained in such a book as this, too brief. It is not impossible that the elementary or secondary-school teacher would contribute more effectively to the mental health of pupils if she were never to hear of the Oedipus complex or the self concept.

THE book is not limited to a discussion of basic concepts, of course. It has examples of guidance practices in school settings sprinkled throughout. Many of them are case studies that illustrate the implementation of the guidance concepts rather than dissected discussions of specific techniques.

Nor do the authors ignore the typical topics found in books with similar titles. They pay some attention to student appraisal procedures, to counseling, and to occupational information. In discussing these topics, the authors use the standard references and cite the well-known people in the field. Their discussion of student appraisal procedures is excellent, but they did a poor job with the topic of group procedures when they ignored several common group procedures. Their discussion of counseling suffers from a preoccupation with an apparent need to compare Rogerian counseling with other forms of the art. Their chapter on vocational information contains many excellent sources of occupational and educational information, useful for practicing counselors but of minimal value to classroom teachers. The topic of vocational development is approached almost entirely from the standpoint of Super's set of propositions on this topic.

Those persons interested in the role of psychology in the preparation of school counselors will find in the contents of this book much to reflect upon. The volume may be commended as a stimulating variation from the typical pattern of content found in basic guidance books.



*A little learning is not a dangerous thing to one who does not mistake it for a great deal.*

—WILLIAM ALLEN WHITE

## Motivational Theory Inchoate

K. B. Madsen

*Theories of Motivation: A Comparative Study of Modern Theories of Motivation.* Copenhagen: Ejnar Munksgaard, 1959. Pp. 352.

Reviewed by BENBOW F. RITCHIE

*who is Associate Professor of Psychology at the University of California in Berkeley. Originally he was a Bertrand Russell and C. W. Morris man, until Edward Tolman introduced him to rats and all the different kinds of learning without wholly weaning him away from the Russell-Morris insistence on rigorous logic, at which Tolman was also so good. He has been at Swarthmore, but now for a decade at Berkeley.*

IT is clear from Madsen's book that many psychologists have for a long time thought that the way to discover how states of the organism influence its behavior is to begin by creating a 'theory of motivation.' The Madsen book contains 337 pages of text and each of these pages contains on the average 250 words quoted from psychologists that Madsen believes have a theory of motivation. He thus presents quotations totalling close to 85,000 words, and they constitute evidence that there is a widespread belief that a theory of motivation is both possible and useful.

According to the book's preface K. B. Madsen teaches at the State Teacher Training College of Emdrupborg, in Copenhagen, Denmark. As a student in Denmark he worked mainly with two psychologists, E. T. Rasmussen and E. Rubin, and with a philosopher and logician, Jørgen Jørgensen.

It seems unlikely that any one else will do what Madsen did in preparation for this book. To begin with, he read and abstracted everything on the topic of motivation that has been written by the following: McDougall, Tolman, P. T. Young, G. W. Allport, Lewin, Murray, Hull, Hebb, Tinbergen, McClelland, Frenkel-Brunswick, Mas-

serman, Freeman, T. V. Moore, Maier, R. B. Cattell, T. M. French, Stagner and Karowski, Skinner, and Holt-Hansen. In addition he read and abstracted everything he could find that was even remotely relevant to his topic in all the psychological journals and in many books and papers dealing with the philosophy of science.

The book he produced after these labors will be a disappointment to those who expect such Herculean scholarship to clarify our understanding of behavior. Instead of writing a critical analysis of the confusions and ambiguities that cloud the minds of those who create such "theories of motivation," Madsen swallows these camels while he strains at the gnats of what he calls "meta-theoretical psychology." In meta-theory, it appears, we seek to classify all such theories properly, and Madsen, following precepts laid down by recognized authorities in the sociology of knowledge, classifies each theory by asking of it the following six questions:

Is it more or less deductive? Is it more or less reductive? More or less behavioristic? More or less molar? More or less field-theoretical? More or less deterministic? No one, however, who has followed Madsen to this point can fail to wonder why he did not also ask: Is it more or less nonsense? Fortunately the book can make its contribution simply by raising this question in the mind of every careful reader.

Let us turn for a moment to the problems we face in studying how states and dispositions influence behavior and ask what relevance a so-called "theory of motivation" has for these problems. We know many perplexing facts about the behaviors of hungry, thirsty, lecherous, frightened, and ambitious animals. What help can we expect from a definition of *motivation* designed to include all these states? Such definitions are the first step in all theories of motivation, and, if they are vague and ambiguous (as they must be in our acknowledged ignorance about the properties of these

different states), then every statement in the theory based on these definitions must be at least as vague and ambiguous as they are.

If there is any doubt that we are ignorant about the primary characteristics of these different states, consider what we say when asked: What distinguishes hunger from thirst? What distinguishes each of these from lechery? And, supposing we can give some clear way of distinguishing these states, what can we then say when asked: What do these three different states have in common that is named by the generic term *drive*? Only when we know enough to answer these questions decisively will we be prepared to define *drive* and thus to distinguish those states that are drive-states from those that are not.

Madsen's book holds the mirror up to psychological theory, and I hope that what we see in this mirror will make us stop primping, and begin to ask some questions we are prepared to answer.



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## Psychophysiology Not Quite Fused

Richard K. Overton

*Thought and Action: A Physiological Approach.* New York: Random House, 1959. Pp. 116. \$95.

Reviewed by RONALD MELZACK

who is Assistant Professor of Psychology at the Massachusetts Institute of Technology. His PhD is from McGill University where he got his knowledge of physiological psychology and his interest in it. He has worked on pain at the University of Oregon Medical School, on scared ducks at University College in London, and he has studied with Moruzzi at Pisa.

THIS small volume, bound in a paper cover, will present an enigma to many psychologists. It took three readings, each with a different 'set,' before the present reviewer could decide at what segment of the population the book is aimed and what criteria, therefore, would be reasonable for assessing it. Its elegant title, together with the fact that it is one of a new Random House series called *Studies in Psychology*, hints strongly that an original theoretical contribution to psychology lies between its covers. With this expectation in the reader's mind, the book is a disappointment.

*Thought and Action* consists essentially of two sections. The first deals with thought, memory, insight, and attention, with the concept of the reverberating circuit looming large; the second section deals with drives, emotions, and the autonomic nervous system. Overton's approach to thought and action, then, has been to put Hebb's neuropsychology and a modified drive theory (with the three faces of Hull, Miller, and Mowrer) side by side, without bringing about any significant interaction between the two. He prefaces his study by saying that he "seeks to unite the field of psychology by giving it a biological foundation." He may have



succeeded in putting two major theoretical systems in intimate proximity under the same cover (and each is certainly biologically structured), but the thought has not quite led to action, and the book ends without the advent of any novel conceptual offspring. What does emerge are eleven General Statements: the tentative assumptions which Overton considers to be the skeleton of his study, a skeleton with little characteristic identity, a hybrid without vigor.

**S**INCE the book is not an original contribution, this reviewer re-read it with a second 'set': Perhaps, he thought, Overton really isn't trying to present original ideas, but has written a study of psychology to be used as an adjunct to a standard eclectic psychological text. Now one begins to see the value of the study. Its tone is strongly behavioristic, and it attempts to present the hard core of a physiological approach to psychology in an elementary way. The author usually sticks to his biological guns, though he falters here and there, as in the case of his discussion of juvenile delinquency. An interesting aspect of the volume is that Freud is not mentioned once in either the text or the suggested readings!

For its strong biological approach and for the enthusiastic (sometimes over-enthusiastic) way in which it is written, the book is commendable. Overton, who is assistant professor of psychology at Fort Hays Kansas State College (he received his PhD three years ago from the University of Texas), is clearly convinced that the physiological approach to psychological problems, both experimentally and conceptually, is of great value, and some of his enthusiasm might come off on the novice reader in psychology to encourage him to pursue further readings in the field. Since the book only takes a few hours to read, lecturers might like to prescribe it as an outside reading to stimulate discussion in conferences and seminars.

As a popularized and elementary account of psychology, it illustrates some of the perils of popular writing. For example, in order to make the reverberating circuit sound as attractive and plausible as possible, Overton sometimes gets carried away with the idea (despite a

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paragraph urging caution to the readers) and such statements as "stimuli cause reverberating circuits, which bring about immediate memory" make this hypothetical construct take on the aura of established fact. The reverberating circuit can be a valuable conceptual tool, when it is not used to hide our continuing ignorance of the real nature of the 'engram.'

The one original contribution in the book similarly loses its value by its popularized treatment. Overton gives a general description of his own experiments which show that rats fed a high-calcium diet make more errors in re-learning a maze than rats fed a low-calcium diet. His vivid and enthusiastic style induces him to describe the experiment as "trail-blazing tests. . . . For the first time in history an experiment had shown that a special (but healthy) diet could produce forgetting! Here was indirect evidence of the vital role of calcium in learning and forgetting." This comment stands in striking contrast with the earlier, much more cautious and modest statements Overton made in the original paper: "There are at least three alternative explanations of the results . . . additional research will be necessary to clarify calcium's role (if any) in learning" (*J. comp. physiol. Psychol.*, 1958, 51, 697-699).

It was the third 'set' with which this reviewer again approached the book that made it appear at its best: the expectation of nothing more than a good exposition of a physiological approach to psychology written in elementary form for the intelligent layman. When considered in the context of other popular paperback books (see C. S. Calvin's review in *CP*, Sept. 1959, 4, 262-267), it is decidedly a worthwhile volume. The material is difficult, and Overton achieves a good balance between the physiological constructs, the behavioral data, and anecdotes to give meaning to both. After reading Overton's study, the average reader should be able to grasp some of the fascination of a physiological approach to psychology. The misrepresentation of a small portion of the material is unfortunate, but it need not damage the exposition for the lay reader; perhaps for the layman an in-

teresting, enthusiastic presentation at the expense of some theoretical detail does not represent an appreciable loss.

## School Guidance at All Levels

Herman J. Peters and Gail F. Farwell

**Guidance: A Developmental Approach.** Chicago: Rand McNally, 1959. Pp. 507. \$6.00.

Reviewed by EDWARD C. ROEBER

*who is Professor of Education at the University of Michigan, where he directs the Counseling Laboratory and a supervised counseling 'experience' for graduate students. He has been deep in counseling, in counseling counselors, and in teaching guidance for many years.*

SINCE book titles are sometimes as misleading as movie titles, it became a challenge to examine "A Developmental Approach" to guidance to see if the use of the term *developmental* implied something new, some rationale perhaps neglected by other introductory textbooks. Chapter by chapter the search began.

It became increasingly apparent that Peters and Farwell, good professors that they are, chose not to define the "developmental approach." Instead they have implicitly described guidance as it pervades all activities and all levels of the educational process. Both authors have worked with teachers and beginning counselors sufficiently long to know that 'actions speak louder than abstract definitions.'

Although most basic texts give at least lip service to the value of guidance as permeating the classroom and all other school activities at all grade levels, Peters and Farwell have perhaps captured the philosophy, psychology, and practices of a longitudinal approach to guidance functions as well as or better than other basic texts. Many of their descriptions and suggestions are as applicable to the first-grade teacher as to the twelfth-grade English teacher.

They show applications in working with parents, with respect to transportation in a consolidated school district or in utilizing community resources. Developmental guidance is developed from A through Izzard.

One paradox (which is not exclusively a characteristic of this volume) is the persistent emphasis upon the developmental needs and tasks for "well-adjusted" or normal-range pupils. Now in terms of printed pages, what do authors ordinarily stress? Deviant pupils and their behavior, of course. Thus here in one chapter about deviant behavior there are 18 pages, whereas the discussion of the "well-adjusted" pupil consumes less than three pages. This criticism may be unfair to Peters and Farwell because they do, indeed, make a valiant effort to stress positive behavior. After all they are reflecting only the paucity of knowledge regarding 'normal' children.

One other inconsistency is apparent in the volume. Counseling per se is given but cursory treatment. The authors say this subject should be handled in a course on advanced theory. If that is right, why not make the same judgment about psychological assessments, statistical concepts, or behavioral descriptions? In particular, do the lists of tests and their descriptions (pp. 190-206) serve questionable ends when the interpretation of test information is so naively covered in but five pages.

Nevertheless Peters and Farwell have demonstrated that it is possible to present the principles of guidance in a systematic fashion, emphasizing how the teachers and others can better meet the needs of pupils through applications of the principles. It is also in their favor that they recognize that teachers are not chameleons, that they do not change into professional counselors merely upon exposure to an introductory textbook.

The emphasis of this volume upon community determinants and resources, group behaviors and procedures, cooperation with parents, and a team approach to guidance services are but a few of its praiseworthy features. It concentrates upon human needs and behavior as they influence applications, a welcome relief from texts grounded upon practices alone.

# The New Tool

Fred Attneave

***Applications of Informative Theory to Psychology: A Summary of Basic Concepts, Methods, and Results.*** New York: Henry Holt, 1959. Pp. viii + 120. \$3.75.

Reviewed by HENRY QUASTLER

who is Senior Radiobiologist in the Brookhaven National Laboratory on Long Island. He has an MD from the University of Vienna of 1932 and has spent most of his professional life practicing medical radiology. In 1951 he organized the Bio-Systems Division at the Control Systems Laboratory at the University of Illinois, and presently became interested in man's informational capabilities. In 1956 he joined the staff of The Brookhaven National Laboratory, and now he uses the new tool of information to study the biological effects of radiation and certain other problems in theoretical biology.

As many readers of *CP* will know, the decisive event in the current evolution of information theory was the publication of a pair of articles in 1948 and 1949 by C. E. Shannon on the "mathematical theory of communication." These papers dealt with the commodity which communication engineers provide for the public: vehicles which can be made to carry information. The theory relates, in an abstract mathematical fashion, general properties of the vehicles with their ability to carry information and deals with the effects which various operations on the vehicles have on the information carried. The articles are written in a highly technical language, they make very difficult reading, and they contain no explicit promises of application to any problems except those of a communication engineer. Yet Shannon's papers became the focus from which applications of information theory spread rapidly in several directions, with men like Norbert Wiener, Warren McCulloch, and Warren Weaver providing the initial momentum. The reason is that the 'ability

of a vehicle to carry information' depends on its specific structure, and, therefore, the mathematical description of said ability is a measure of the degree of specificity, structure, organization of the vehicle, and indeed not only of a vehicle destined to carry information but of any thing or event. Here, then, was a mathematical theory which promised to furnish tools for a rigorous mathematical treatment of phenomena like order, shape, organization—and what's more, the basic mathematical function is one which physicists have been using to define entropy, a concept difficult to understand but certainly related to order.

The wave of enthusiasm rose high and produced some very good results, but also, unavoidably, some rather rash, over-imagined and under-reasoned statements out of some of the more volatile members of the scientific community. The reaction was swift and violent; it became almost embarrassing to use information theory in psychology. As a result, serious psychologists still interested in the new tool felt a great urge to separate themselves carefully from the lunatic fringe. Several authors (this reviewer included) spent much time showing what information theory should NOT do or be expected to do, and that, used with proper precautions, it really is a respectable mathematical theory, closely related to certain branches of statistics which the fraternity had learned to accept, a theory which can be used in connection with such well-established laboratory activities as key-pressing and tachistoscopic presentation. So treated, the applications of information theory become perfectly legitimate and acceptable and also, sometimes, a little bit dull.

THE reason why Attneave's book brings forth such thoughts is precisely that it is such a very good book. Those who have read Attneave's research papers know that he can provide a most successful blend of solid science with artistic elegance. (For those who do not know him, here are his vital statistics: BS, Mississippi, 1942; PhD, Stanford, 1950; officer, Signal Corps; scientist, Air Force Personnel and Training Research Center; fellow, Institute for Advanced

Study in the Behavioral Sciences; now associate professor of psychology, University of Oregon; consulting editor, *J. exp. Psychol.*) Attneave has utilized both his scientific and artistic talents in providing an eminently readable introduction to information theory for psychologists. The largest portion of his book consists of a very skillful derivation of the various mathematical functions which define 'amounts of information.' The treatment is purely heuristic and should succeed in establishing a good intuitive grasp of the basic concepts of information theory. This is where the book's value lies, not in the sugar-coating of a little mathematical formalism since most psychologists (experimental psychologists, anyway) are equipped to follow much more rigorous mathematical arguments. The reader who works his way through the book should find that he is able to actually use information theory in investigations of the kind described (in the smaller portion of the book). Incidentally, he will also find tables which he can use in computing information measures.

At this juncture, the reader might feel a little disappointed. He used to know that printed English was somewhat redundant; now he is told that a conservative estimate of this redundancy is 75%—but he will not necessarily know what to do with this number. He used to know that it helps if a story is told in more than one way; he will now find out that in a particular experiment Ss given the same information in three different ways absorbed 4.1 units of information while they got only 2.7 units worth if only one way of presentation was used; but he will not necessarily see how this and similar quantifications add up to a coherent picture of man's ability to process information. The reader might feel that the powerful tools of information theory could possibly be applied to weightier tasks; if he does so, he will be sharing the private convictions of many workers in the field.



*The very technology that makes our living simpler makes our society more complex.*

—THOMAS GRIFFITH

# The Fraudulent Title

Allan D. Bass (Ed.)

*Evolution of Nervous Control from Primitive Organisms to Man.* (A Symposium organized by the Section on Medical Sciences of the American Association for the Advancement of Science and presented at the New York Meeting, 29-30 Dec. 1956.) Washington, D. C.: American Association for the Advancement of Science, 1959 (Publication No. 52). Pp. viii + 231. \$5.75 (\$5.00 to AAAS members).

Reviewed by ROBERT A. MCCLEARY

who is Associate Professor of Psychology at the University of Michigan and Consultant to the Army's Surgeon General. He has a PhD from Hopkins and is interested in the kinds of things that would have led him to buy this book had not CP sent it free to him for review. He reviewed Abrahamson's *Problems of Consciousness for CP* (Aug. 1956, 1, 243f.).

THIS book turns out to be afflicted with a disorder to which we are increasingly exposed: it has a fraudulent title! Really it is a collection of nine but slightly related articles—each with something to do about the nervous system or behavior—that were presented at the meetings of the American Association for the Advancement of Science in December 1956.

Of the nine articles, only two of them (by C. L. Prosser and H. Gundfest) have any claim for being in a book on evolutionary development. These authors discuss current conceptions of nerve-fiber conduction and neural networks and, fortunately for the title, draw heavily upon neurophysiological experiments involving an assortment of animals from various levels of the phylogenetic scale.

Two of the remaining seven articles manage a phylogenetic ring in their

separate titles by appropriate use of the words *development* and *comparative*. The actual contents of these articles by E. W. Sinott and J. V. Brady, however, are really concerned, respectively, with a teleological approach to the mind-body problem and the effect of a tranquilizer on emotional behavior in the rat and monkey.

The remaining five articles by M. C. Niu, G. B. Koelle, I. H. Page, H. L. Tueber, and I. A. Mirsky make no pretense in either title or content to having anything to do with the *Evolution of Nervous Control*. These remaining articles, respectively, present straightforward considerations of chemical inducers in embryological development, chemical transmission at synapses, the history of man's interest in brain chemistry, the behavioral effect of cerebral lesions in man, and a selected review of some laboratory experiments bearing various degrees of relationship to psychoanalytic theory.

This reviewer would have felt cheated if, on the basis of the title, he had ordered and paid for the book.

## Mobility of Status and Ideas

Pitirim A. Sorokin

*Social and Cultural Mobility.* (Rev. ed.) Glencoe, Ill.: Free Press, 1959. Pp. xv + 645. \$7.50.

Reviewed by F. K. BERRIEN

who is Professor of Psychology in University College at Rutgers State University. He is the author of *Practical Psychology* (Macmillan, 2nd ed., 1951) and, with W. H. Bash, of *Human Relations* (Harper, 2nd ed., 1957). Just now he is busy testing hypotheses about the homeostasis of small groups.

THIS is not a new book. In 1927 and again in 1941, Pitirim Sorokin published his now famous *Social Mobility*. Under the imprint of still a third publisher we have *Social Mobility*

again, this time supplemented by two chapters from another of Sorokin's books, *Social and Cultural Dynamics*. The publishing mobility of this book is remarkable!

In 1927 Sorokin's thesis was new. He proposed that the ebb and flow of people within and across social class-lines can be accounted for by certain demographic, physiological, and psychological characteristics. Almost no sociologist before Sorokin was interested in social mobility as a problem for serious study in the broad sweep that Sorokin defined. Perhaps his interest was kindled by the rapid social mobility of the Russian revolution in which he participated and in which he served briefly as prime minister of the All Russian Peasant-Soviet. Exiled later from his native Russia, he eventually joined the Harvard faculty in 1930 and ever since has been a provocative, sometimes a controversial, figure in sociology, espousing propositions which some would describe as original and others might say are 'off beat.' He is currently Director of the Research Center in Creative Altruism.

In any event, the book was remarkable not only for its highly readable style but also for its wide-ranging scholarship and historical grasp. Before Warner proposed the upper uppers, the lower uppers . . . and the lower lowers, Sorokin set up definitions of social stratification in social space as distinguished from geometrical space. People moved from one social stratum to another or retained their positions because they possessed the requisite intelligence, certain gene-determined physical characteristics, or had the proper family connections. Perhaps of secondary importance were learning and membership in the 'right' church. History is essentially trendless, and this point is reiterated almost tirelessly; history is cyclical. Societies grow and decay. Long-term views of society show, according to Sorokin, no over-all improvement. In periods of decay the élite change from strong, ambitious, adventuresome non-sentimentalists to soft, timid, humanitarians who either through revolutions or by lawful means are displaced by persons more like the earlier élite.

In the appended chapters from *Social and Cultural Dynamics* Sorokin at-



tempts to account for the mobility of cultural objects and values, just as he had earlier accounted for the movement of people within and across social strata. Art, religion, language, and law are dispersed along two-way channels of communications. In periods of cultural growth these value-objects flow from the upper classes to the lower as 'finished products' but in periods of decay the stream flows in the opposite direction.

THE historical and empirical evidence supporting these propositions is open to serious question in many instances and has been the source of debate among sociologists over the years, but it is equally important to question the implied cause-and-effect relations which are affirmed again and again throughout the book. In support of his thesis of social mobility Sorokin presents tables, for instance, of the height and weight of Italian children separately for poor and wealthy families. He draws the implication that the elite are such because they are better physical specimens. In connection with mobility of cultural value-objects and the contributing causes of social decay, he quotes from historical sources to the effect that Nero, Petronius, and Messalina "developed a curious taste for low life that reaks and festers in taverns and stews. . . . The distinguished dinner party with the Emperor at their head sallied forth to see how the people were living in the slums." Again one wonders whether this is merely a description of social decay or whether, as Sorokin suggests, it is evidence of the reversal in the flow of 'finished' products that he proposed as the cause of social decay.

Reading this book in the context of 1960 rather than 1927 one is impressed by the differences in research methodology and qualification then and now. The conclusions are sweeping, the data suggestive but questionable by 1960 standards. The book today has value as a historical document in which are funneled some 700 references that were available and pertinent to the pioneer student of shifting societal composition.



## Chimerical Chemistry

Donald E. P. Smith and Patricia M. Carrigan

### *The Nature of Reading Disability.*

New York: Harcourt, Brace & Co., 1959. Pp. viii + 149. \$6.00.

Reviewed by THOMAS W. RICHARDS

who is Professor of Psychology of the Department of Psychiatry and Neurology of the Louisiana State University's School of Medicine. He first touched reading disability in the Psychological Clinic at the University of Pennsylvania in the days of Lightner Witmer; then at the University of Iowa he got into the physiology and biochemistry of behavior, and after that into factor analysis at the Fels Research Institute at Antioch College. In 1946 he published a book, *Modern Clinical Psychology* (McGraw-Hill).

FROM the title and mode of organization of this book, the reader might infer that it is a monograph. Actually these two young specialists in remedial reading report in this volume three diverse activities of theirs, which they try to work into a whole. Let us take them one by one. The main topic is, of course, reading disability.

The most important contribution in this little book is a factor analysis of the performance of two hundred children, primarily poor readers, on a variety of psychological tests: flicker fusion, intelligence (vocabulary, word naming, mazes, Knox cube, digit span, coding), spelling, introversion-extraversion, stability, auditory and visual discrimination and acuity, and visual blending. The authors do not present the matrix of original intercorrelations, a serious omission in such a report. Apparently, three factors accounted for most of the variance; (1) high loadings on verbal fluency, coding, and perceptual speed, (2) mazes and the Knox cube, and (3) instability, poor auditory discrimination, and poor performance with identical forms. So far, so



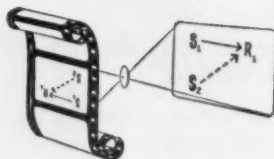
good. But the authors interpret these as (1) a "cholinesterase dominance or rapidity" factor, (2) an "acetylcholine dominance or performance accuracy" factor, and (3) an "anxiety" factor. While there may be some justification for interpreting a high instability score as anxiety, with no direct measure at all of acetylcholine cholinesterase functioning in the battery of tests (much less among the tests with high factor loadings), their designations of factors 1 and 2 are inexcusable.

With less pretense of scientific objectivity, the authors' second contribution is a clinical investigation of forty severely retarded readers, in which they used the same battery of psychological tests together with a study of the health and physical characteristics of the children. Here some tests of blood chemistry were made. The authors here utilized a method of inverse factor analysis in which each of the twenty children was correlated with the others. Again, however, they make unjustified interpretations of the types of children emerging from the analysis, designating them in terms of acetylcholine and cholinesterase balance, with no direct measure of this metabolic activity. They do show the children who are very poor readers and their families tend to have a higher incidence of metabolic disorders than might be expected in a normal group.

THE third presentation by these difficult authors is an extended discussion of a 'model' of synaptic transmission (again via acetylcholine and cholinesterase), the disruption or dysfunction of which may explain the nature of reading disability. This theory they present with an admixture of elementary endocrinology and tenuous scientific evidence. Reading disability, they think, is very much a medical problem, and remedial training should be supplemented with drugs or vitamins or both. This conclusion may be justified by their clinical experience, but it is surely not a valid conclusion based on any of the findings reported in this book.

# INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



## A COMMERCIAL MASS MEDIUM BECOMES LESS SO

Carroll V. Newsom (Ed.)

*A Television Policy for Education.* Washington: American Council on Education, 1952. 266 pp., \$3.50.

*Credit Courses for Television.* Washington: American Council on Education, 1955. 49 pp., \$1.00.

*Teaching by Closed-Circuit Television.* Washington: American Council on Education, 1956. 66 pp., \$1.00.

John C. Adams, C. R. Carpenter, and Dorothy R. Smith (Eds.)

*College Teaching by Television.* Washington: American Council on Education, 1958. 234 pp., \$4.00.

Reviewed by RICHARD I. EVANS

Dr. Evans, who is Professor of Psychology at the University of Houston, received the first PhD in psychology (1950) granted by Michigan State University. His interest in educational TV was whetted when, in 1953, he became the first professor to offer a TV course in psychology on a noncommercial station. Many CP readers have read Time Magazine's account of his filmed interviews with Carl Jung and Ernest Jones. Currently, Dr. Evans telecasts twice a week on psychology, conducts research on instructional media under a USOE grant, and is Chairman of the APA Division 2's Committee on Educational Television.

THESE four reports reflect the continuing efforts of the American Council on Education to disseminate ideas about the new instructional media.

Each report is the record of a conference, held during the period from 1952 to 1957, which dealt with some phase of the new instructional medium, television. Since the use of television in education is so recent, these reports also document incidentally a significant portion of the history of the educational television movement.

The first of these publications, *A Television Policy for Education*, reports the proceedings of a conference held at Pennsylvania State University in 1952, in which educational leaders, researchers, telecourse instructors, and television technical experts participated. Since the 242 television channels reserved by the Federal Communications Commission for noncommercial educational use were to be held for this purpose only until June, 1953 (the time limit was subsequently extended), the conference took place in an atmosphere of urgency. A general awareness was expressed of the opportunities these channels offer for the development of a significant educational movement, and discussion centered, therefore, on ways and means—financial, technical, and pedagogic—of fully utilizing the resources of television for educational purposes.

The second of these reports, *Credit Courses by Television*, is the record of a conference held at Michigan State University in 1955. Since by this time telecourses had been offered for credit by many more institutions than in 1952, it appeared that an exchange of ideas among those experienced in the administrative and technical aspects of television instruction would be fruitful. Specific problems were dealt with in the area of institutional and departmental policy, including questions of costs and revenues, enrollment and registration

procedures, and faculty schedule and compensation. The television administrators present appeared to be preoccupied with promoting instructional television in terms of the economic advantages which would accrue to the institutions offering telecourses. In fact, an emphasis on economic advantages rather than on intrinsic educational values may still be so prevalent as to interfere with utilizing television instruction for the most fundamental, long-range contribution to education.

In 1956 a third conference, reported in *Teaching by Closed-Circuit Television*, was held at the State University of Iowa. The participants included persons from the academic world who were interested in the operational aspects of television, individuals from content areas in which telecourses were being offered, researchers in the field, and, again, administrative leaders in the educational television movement. This conference, to an even greater extent than the 1955 conference, was based on actual experience in offering telecourses. An attempt was made to evaluate the results of this experience, and some of the participants argued for the greater efficiency of teaching by television. Selection of TV curricula, teaching methods, and faculty and student attitudes were considered at greater length than in either of the earlier conferences.

The most recent of these publications, *College Teaching by Television*, reports the proceedings of a conference held at Pennsylvania State University in 1957. Over a hundred professors experienced or interested in television instruction attended this conference, the most comprehensive and the best organized of the four. They represented colleges and universities throughout the United States, as well as administrative leaders in the educational television movement. Papers and discussions ranged from anecdotal reports of individual experience in offering telecourses to critical evaluations of such courses. Certain topics treated only cursorily or not at all at the earlier conferences received attention here. Neal Miller, for example, presented a paper discussing principles of learning as related to television instruction in the "drive-cue-response-reward" framework. Hideya Kumata summarized re-

search findings on the comparative effectiveness of TV and traditional courses as measured by student achievement and attitude tests, which in general indicated no significant differences in achievement but often marked differences in attitudes. Clarence H. Faust, President of the Fund for the Advancement of Education, attempted to appraise instructional television in the light of educational philosophy, emphasizing the importance of quality presentations. The volume also includes a number of supplementary papers prepared in advance of the conference and distributed there in mimeographed form.

AMERICAN psychologists have been active in the educational television movement since its beginning, and some of them participated in the planning of these conferences. Among other psychologists participating at these conferences were those who had done research in this area or had presented psychology telecourses.

The substance of these reports strongly indicates the need for further scrutiny by learning theorists and social psychologists, who are too often disinterested in education, not only in television as an educational medium but also in educational philosophy and teaching methods in general. In fact, in the opinion of this reviewer, one of the most important impacts of the ETV movement is that it has, perhaps defensively, required a re-examination of teaching behavior in general and a re-evaluation of its effectiveness.

Although there was no general agreement among participants at any of these conferences on all points, nor, perhaps, complete agreement on any point, these reports indicate certain results typical of present experience which might be helpful to a department interested in offering a telecourse.

(1) In a large-enrollment, introductory course, student achievement tests indicate that teaching by television is at least as effective as teaching by more traditional methods.

(2) It appears that instructional television, if properly utilized, could contribute to a more efficient use of professorial time. Since more students can be taught in one television presenta-

tion than in one classroom presentation, the teaching load of faculty members could be reduced, thereby increasing the amount of time available to them for research.

(3) The planning and implementation of a telecourse frequently leads to a more careful consideration of course content and teaching techniques than is usual in a normal course presentation. In other words, preparing a telecourse can be a means for the improvement of teaching, whether it reveals itself as such in the immediate achievement of the student or not.

Not all of the indications are equally encouraging, however. On the more pessimistic side, these reports suggest the following typical problems.

(1) Many professors feel that the content of telecourses is usually 'watered-down' and are averse to them for this reason.

(2) Fear that the widespread use of television may result in technological unemployment is another source of negative attitudes of some professors toward ETV.

(3) Many professors have serious doubts that the traditional student-teacher contact, which is lost in TV presentations, can be adequately compensated for.

(4) Many students would certainly

resent being compelled to take courses by television.

In conclusion, it may be pointed out that, in the opinion of this reviewer, if these four reports are examined at face value, they would have to be described as loosely organized, containing papers and discussions of very unequal value, and of little technical interest to psychologists. Furthermore, if they are to be regarded in the light of being rigorous contributions to the field of education, they fall far short of the mark. They may, however, be interpreted in perspective as a kind of interim historical documentation on the unfolding of a significant educational social movement. This movement is already affecting the field of education to a pronounced extent for there are over 250 closed-circuit TV facilities in educational institutions and about 50 open-circuit ETV stations which are already operating. ETV will in the future probably affect education even more under the aegis of such agencies as the U. S. Office of Education and the Ford Foundation which can provide financial support. Thus these reports should be of interest not only to most psychologists but to anyone interested in the educational process and the implications of new techniques for the growth and dissemination of knowledge.



*Among the varied interests that complained to one [television] network . . . were: Dentists over a dentist causing pain in patients, warehousemen because they felt that nine times out of ten murders committed on the air involved a warehouse, gas companies on the use of gas ovens in suicide attempts, toupee manufacturers about jokes dealing with toupees, securities dealers over the presentation of crooked securities dealers, leather manufacturers on the presentation of juvenile delinquents in leather jackets, pharmacists over the presentation of incompetent pharmacists, dry cleaners on the dry cleaners shown on television who leave spots in a garment, and waitresses about television waitresses who are hardboiled and tough.*

—CHARLES WINICK



# ON THE OTHER HAND



LYKKEN ON EYSENCK

Let me congratulate Professor Eysenck on his retort (*CP*, Mar. 1960, 5, 106f.) to my review of his *The Dynamics of Anxiety and Hysteria* (*CP*, Dec. 1959, 4, 377-379). One must acknowledge him a master in the art of coping with critics, an example of the keen edge which practice gives to native wit.

Art aside, I find less merit in the substance of his rejoinder. For example, I had expressed surprise at the satisfaction which he took in a correlation of .29 between measures of extraversion and reminiscence, offered in the book as the main support of a major deduction from his theory which holds that both variables are a function of "cortical inhibition." Now he adds that one of these measures was "not a very reliable score," but why he finds comfort in this admission is not clear. I am not aware of any "fundamental principle of psychometrics" which requires one to take a trivial correlation more seriously when it involves unreliable measures. It seems cruel to abuse the correction for attenuation in this way.

Eysenck continues to maintain that the Franks data support his prediction that introverts should "form conditioned reflexes quickly and strongly" as compared to extraverts, with normals falling in between. He dismisses my comment that Franks' curves fail to show differences in rate between the three groups by again appealing to a defect of the experimental measure (which came, like the reminiscence score above, from his own laboratory): "The procedure used . . . does not lend itself very well to a precise analysis. . . ." He also states that "later research has shown that the postulation of higher asymptotes (of inhibition in extraverts) requires qualification," which means that his theory cannot predict differences in asymptotes of conditioning either. Now, if these groups differ neither in rate nor in asymptote, then how they could be said to differ in "quickness" and "strength" of conditioning continues to elude me.

Eysenck's treatment of another of my criticisms is a classic of parry and counter-

thrust. I had shown that the same reasoning, by which he had made his theory predict that extraverts would have stronger perceptual aftereffects, would also predict that they should see the spiral illusion more strongly, and I had complained of his reversing his arguments in the spiral case and claiming this too as an empirical confirmation. Eysenck replies by advancing an entirely new argument based on the 'demonstration' by Ditchburn and Ginsborg and Riggs et al. that "fixation of the stimulus figure produces satiation, which shows itself as a weakening of the percept." Now, in the first place, this research has nothing whatever to do with 'satiation' as the term is used in Eysenck's theory. Using ingenious techniques to immobilize the retinal image in spite of normal saccadic movements, these authors have observed a fading of the perceived pattern into a darkening field followed by a sudden resurgence of the original percept, with the entire cycle taking about 10 sec. and repeating itself over and over. To claim this as 'proof' that persons with more cortical inhibition will find the fixation stimulus to have less "excitatory quality" and therefore will experience less aftereffect, illusion, and the like, is surely foolishness.

But even if there were evidence to support the new premise, the reasoning which follows remains as slippery as ever. If the fixation stimulus does yield more 'satiation' in the extraverts, then they should show less aftereffect in both the spiral and the Köhler-Wallach experiments; in fact, they show (according to Eysenck) less in one case and more in the other. So much for the parry. We find the counterthrust in the next paragraph where I am said to be playing "heads I win, tails you lose" with Eysenck; I would sooner play croquet with the Queen of Hearts.

I am less certain how to deal with Eysenck's complaint about my youth. Perhaps I should concede the point, but with one qualification—I was not born yesterday, Professor.

DAVID T. LYKKEN  
Center for Advanced Study in  
the Behavioral Sciences

## SCHIZOPHRENIA'S IMPORTANT

Almost a year and a half after the book's publication, *CP* has published a review of *Schizophrenia*, edited by Leopold Bellak, a noted clinical psychologist and psychiatrist. As I am familiar with the book in question as well as with numerous other publications by Dr. Bellak, I was incensed by the unseemly tone of this review. Under the Editor's heading of *Psychiatrist's Cornucopia*, the reviewer, Dr. Bernard Saper, sneers at the author's "conversance with psychoanalysis, psychiatry and clinical psychology" at a time when the nation's foremost clinicians are preoccupied with the thorny problem presented by schizophrenia—the subject of this volume.

As a book review editor of another journal, I was astounded that the *CP* editors would have chosen a little-known psychologist to review a book by a clinician of international repute. There is also the question of why the editors let the unnecessarily biting, personal lines in this review escape the red pencil. Could it be that *CP* harbors the bias of many academic psychologists against truly clinical contributions?

I believe there is an editorial responsibility to select appropriate reviewers to be cognizant of the proper importance of a review and to guide its reviewers in these respects.

SAUL SCHEIDLINGER  
Community Service Society,  
New York, N. Y.

## RUBINSHTEIN

A second, hard look refused to dispel the persistent impression of what appears to me to amount to a charge of bad faith in a *CP* book review. The charge seems to me to be contained in the dismissal of a book by the Soviet psychologist, Rubinshtein, as just a communist ritual (Ivan D. London, *CP*, Mar. 1960, 5, 98f.): "Yet the Westerner is tempted to question how seriously such writing is actually taken, after the usual praises have been sung."

The review not only dismisses Rubinshtein's points of view as "elaborately argued absurdities," but also uses what it pronounces as "absurdities" to cast doubt on the integrity of the entire effort and to reduce it to a "fashionable relic." The text of the review implies that the problems of Rubinshtein's philosophical book on psychology are foisted upon Soviet psychologists and interfere with their professional work. The review rules out as beyond question its description of the *status quo* in the West between philosophy and psy-



chology: "Western habit in psychology is to eschew an essential preoccupation with matters deemed within the province of philosophy. . . . Ordinarily we tend to be suspicious of the philosophizing psychologist and begrudge him any useful role." Yet London's description of the *status quo* is not evidence of its desirability. Dallenbach (*Amer. J. Psychol.*, 1955, 68, 525), for example, is puzzled by the great popularity of psychoanalysis among American psychologists and is moved to ask: "Why should clinical psychologists today accept what their confreres of 30 years ago either ignored or denied? I do not know, I wish I did; my guess, however, is that it is due to their lack of training in philosophy."

Once we admit the possibility of the relevance for psychologists of the philosophical problems raised by Rubinshtein, then we are free to weigh the claims of his offered solutions. The review gave no such evaluation. It referred disparagingly to the existence of "elaborate" arguments and of "the inevitable specifics." The arguments and specifics were mentioned only to be dismissed. The peremptory dismissal stripped the book of both. Stripped of arguments and specifics, all that remained in the review was "essentially a *tour de force* in dialectical writing"—to use the reviewer's own phrase.

S. D. KAPLAN

Lincoln State Hospital, Nebraska

*In this Department CP invites discussion of reviews and of books reviewed. Here is the place for that kind of intellectual dissent that promotes progress in understanding. Let your criticism be ad verbum, not ad hominem. Seldom does a criticism merit more than half the space of the text criticized—never more than equal space and then only when the letter is interesting and well written. CP edits letters when it thinks they should be. Single-spaced letters will be returned for double-spacing.*

# HAWTHORNE REVISITED

## *Management and the Worker,*

Its Critics and Developments in  
Human Relations in Industry

By HENRY A. LANDSBERGER

Assistant Professor, New York State School of Industrial  
and Labor Relations

Since its publication last year, *Hawthorne Revisited* has been extensively used in courses in industrial psychology, personnel management, and industrial sociology. Reviews ★ from a wide range of publications indicate its significant contribution to the field of human relations.

- ★ In this short book, Landsberger provides us with perhaps the best summary of the Western Electric experiments, a convenient summary of the criticisms, a brief weaving together of later studies, and valuable leads for further research. It is also distinguished for its clarity of style.

S. M. MILLER

*American Sociological Review*

- ★ Compared with the critics of *Management and the Worker*, Landsberger makes a much broader and more constructive contribution. First, he provides the finest summary available of the *total* research to be found in the work . . . followed by excellent documentary evidence to show that the book made many fine contributions.

The account . . . makes a fascinating, extremely well-written book that not only accomplishes its objectives but may well serve as a model for other authors who evaluate critically an emerging academic field.

CHRIS ARGYRIS

*The American Journal of Sociology*

- ★ It is to be hoped that . . . all those who have compiled, for whatever purpose, reading lists which include *Management and the Worker* will hasten to add Professor Landsberger's book.

PETER CAVANAGH

*Occupational Psychology*

- ★ . . . a must for any student and practitioner of personnel administration . . . a landmark for evaluating what happened to Mayo's ideas after receiving general acceptance.

EUGENE EMERSON JENNINGS

*Personnel Journal*

- ★ A subject interesting to management . . . refreshing . . . controversial.

*New York Times*

- ★ Landsberger is the first writer to stand outside the pro- and anti-Mayo factions and evaluate the controversy. He has presented a persuasive and exceptionally well documented case.

J. H. SMITH

*The British Journal of Sociology*

- ★ With an unusually good evaluation of its assets and liabilities and with clear, interesting writing, Landsberger has helped to set the record straight about *Management and the Worker*.

THEODORE V. PURCELL, S. J.

*Social Order*

- ★ A much needed appraisal. It brings back into balance the character of the research.

DELBERT C. MILLER

*Indiana University*

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Ithaca, New York

Cloth 132 pp.

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